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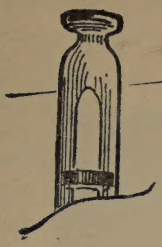

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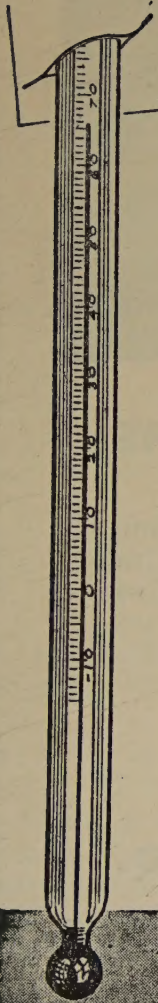

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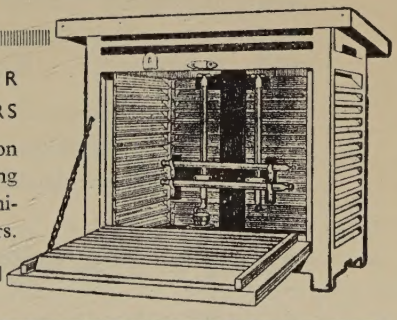

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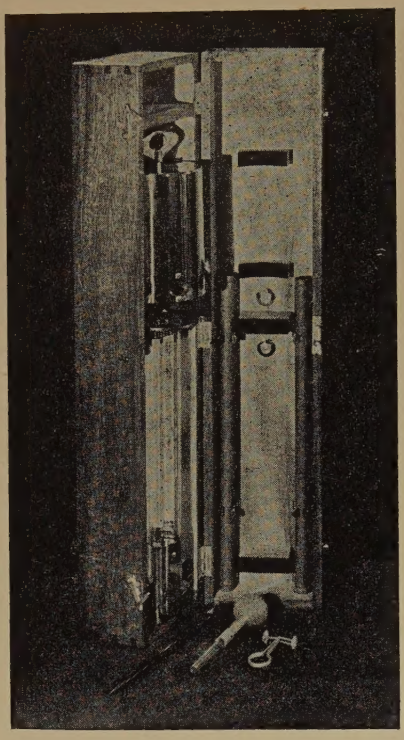

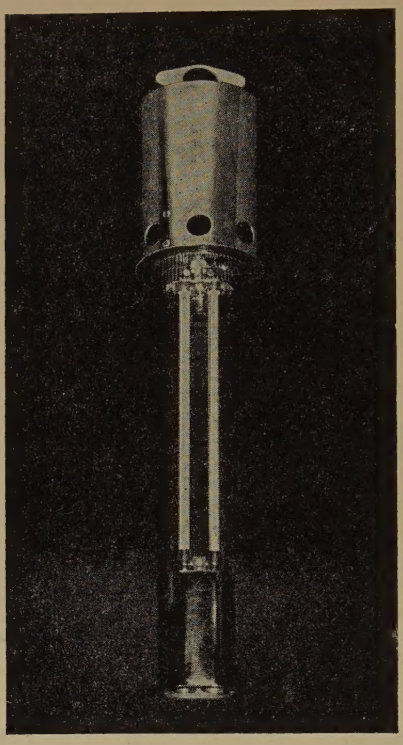

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REVIEWS OF MODERN METEOROLOGY—10

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Convection in the atmosphere

By F. H. LUDLAM and R. S. SCORER
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1. The process of convection

Motion in the atmosphere originates almost entirely through the action of gravity on masses of air that are differently heated. At the one extreme is the motion of the pressure systems and at the other is the small-scale stirring motion, usually called turbulence, which very largely originates through the degeneration of motion of a larger scale. In between is the overturning motion in which the vertical velocities are comparable with the horizontal ones. This kind of motion, which is therefore restricted to dimensions not greatly exceeding the depth of the troposphere, is our main concern. We shall not deal with the synoptic situations in which this overturning occurs, nor with the results except in so far as they affect the motion itself, but we shall see how far this complicated process can be comprehended in simple terms.

The fundamental idea of buoyancy was clearly understood by Archimedes (*c.* 220 B.C.), and although the formulation of Newton's laws of motion made it possible for the science of hydrodynamics to advance enormously, the study of convection has lagged far behind. There are even signs that it might be split into two parts, on the one hand *hydrostatics* and on the other *turbulence*, and most of the concepts used in thinking about convection have been derived from one or other of these. An exception to this is the study of *convection cells*. Most of the work on this aspect of convection has been described by Brunt (1937 and 1939) who gave to Bénard the credit for having first recognized cellular motion and studied it seriously. Since 1939, treatments of convection cells have tended to become irrelevant to the motion in the atmosphere.

Since glider pilots began in the 1920's to develop the art of *thermal soaring* they have been, somewhat unwittingly, the chief source of information about the nature of convection currents, and powered aircraft, in spite of their ability to carry instruments and go to any chosen position, have not yet outstripped gliders as a means to discover more about thermals. If the emphasis of effort in aviation had been on ability to remain airborne rather than on speed and independence of the weather, there would undoubtedly be aircraft in existence today which would be ideal for meteorological exploration. As it is, a convection cloud is regarded by aviators as a sometimes hazardous region of bumpiness and possible icing rather than of interesting and helpful motions.

The study of heated jets attracted much attention in the 1940's, particularly in connection with fog dispersal, and had much influence on meteorological thought, particularly in America which was far from the birthplace of thermal soaring in Germany. Finally, the atomic bomb cloud has provided some ideas on the subject, but it can scarcely be said that it demonstrates anything relevant to our problem that cannot be seen in ordinary cumulus clouds.

Because there has so far been no synthesis, and some contradictions of viewpoint have arisen from these various lunges into the field from one side or the other, our chief aim is to present a unified picture of what happens during the process of convection. We shall not discuss the complications which arise when precipitation is formed inside convective clouds.

2. Slow convection

2.1 *Cellular patterns*

Convection occurring in a cellular pattern has been extensively studied on account of its relative simplicity. The pattern is stationary and the motion steady. The mathematical theory has been concerned with a layer of gas confined between two rigid conducting surfaces or with a shallow layer of liquid. The purpose of the theory has been to find the shape of cell that occurs most easily, and it has been assumed that the temperature gradient through the layer is uniform and is slowly increased until convection begins, and that then the transport of heat is maintained at the minimum required to keep the motion going. Acceleration of the motion is prevented by the molecular viscosity which destroys shear and by the molecular conductivity which destroys horizontal temperature gradients produced by the motion.

Laboratory studies by Walker and others (described by Brunt) have been concerned with the same conditions and the cells have been rendered visible by smoke in gas or metallic dust in liquid. It has been possible by applying a relative horizontal motion to the bounding surfaces of the gas or by having a wind across the liquid surface to arrange the cells in a rectangular pattern or to distort the cells into longitudinal or transverse rolls. Jeffreys (1928) suggested that longitudinal rolls, in which the direction of rotation

in adjacent rolls is opposed, were to be expected; but the transverse ones, all with the same direction of rotation, can be obtained with small amounts of shear. A hexagonal or rectangular pattern can be obtained with no shear.

In the atmosphere, altocumulus formations are strikingly similar in appearance to these laboratory patterns, but there are many differences. If the motion were confined between two almost horizontal surfaces a layer of cloud would appear at the top, and not the separate cloudlets which are observed. One possibility is that the upper or lower surface of the overturning layer is not horizontal and that the cloudlets are in thicker parts. It is evident that in the atmosphere no difference in density comparable with that at the upper surface of a liquid will occur and that the inertia of the overturning fluid cannot be neglected, as it is in the theory, particularly since the restraints of viscosity and conductivity are so much less. Theory and experiment are not in accord concerning the humps and hollows on the upper surface of an overturning liquid (Jeffreys 1951), but the problem with no sharp density discontinuities has not been tackled theoretically and this discord has therefore not arisen in the atmospheric case.

The instability in the atmosphere can arise in a variety of ways, but it is unlikely that a uniform temperature gradient will be slowly established through a cloud layer. It is more probable that the instability will arise at the upper or lower surface of a cloud layer if a general lifting or sinking of the air occurs. In this case, unless the whole depth of the cloud is involved in the overturning and the other surface acts as a sharp limit to the convection, there will be no definite length characteristic of the defining circumstances of the motion and so no characteristic cell size will appear. Indeed, most altocumulus does not possess a uniform cell size. When shear occurs, variations in the depth of the overturning layer will be inhibited and a definite cell size will appear at the same time as regular formations.

The molecular viscosity and conductivity play a vital role in the theory and in laboratory experiments. They enable a uniform temperature gradient to be gradually established, and prevent accelerations when the overturning begins. There is no equally effective agent in the atmosphere where molecular influences are negligible and eddy viscosity and conductivity do not become effective until the motion occurs. The molecular effects also determine the direction of motion within the cells, which in a gas is downwards in the centre and in a liquid upwards. This is said to be because viscosity increases with temperature in a gas, but decreases in a liquid. It is, however, possible that the direction of motion is also greatly influenced by the weight of suspended particles used to make the motion visible, and is predominantly upward in gases containing heavy smoke because of this. The motion in clouds appears to be predominantly upwards in the middle of the cells because cloud lumps are more common, and though reticular (netlike) clouds do appear, it cannot be concluded from their appearance that downward motion in the middle is producing the evaporation of the cloud, for the holes may be due to the perforation of the cloud layer by a clear layer below.

If we examine the atmosphere to see if anywhere the conditions of the theory and laboratory experiments might be more nearly approached than in altocumulus cloud, the region of the NE. Trades seems a likely place. The air is moving steadily towards warmer sea so that heat is being put in slowly at the bottom and the motion is often limited at the top by a sharp inversion. Woodcock and Wyman (1947) claim that cellular convection does occur there but the evidence is suggestive rather than compelling, and the motion is not, for the reasons given, completely analogous to that discussed in the theory and observed in the laboratory.

The complication due to condensation has not been considered : cloud has merely been treated as an indicator of the motion. It is possible that the overturning is ' once

for all ' and that the cloud lumps are the air masses that have ascended (or descended if the layer is penetrated by clear air from below in reticular fashion). This obviates the difficulty that steady cellular motion produces a cloud layer, but the real nature of the motion can only be discovered by a more careful examination of the clouds themselves than has yet been undertaken.

2.2 *Billows*

The longitudinal rolls which seem most likely to occur according to laboratory experiment and theory have not been definitely observed, except possibly in cloud streets (to be discussed later), whereas transverse rolls are common in clouds. If the motion is not ' once for all,' but overturning continues, then the appearance of gaps between the rolls suggests the heaping together of a layer into the rolls. If this were once effected then it has been shown (Scorer 1951) that the configuration can be described mathematically, the billow-like structure being perpetuated by wavy motion above and below the overturning layer. The amount of shear that exists does not affect the spacing of the billows which depends only on the depth of the layer overturned. The theory only applies strictly to infinitely long rolls and when there is an unending row of them.

The dynamical effect of the wavy motion in the air above and below seems to have the effect of spreading the regularity of spacing beyond the region where the instability originally began. Thus if a patch of cloud becomes unstable and forms billows, further billows often appear in clear air with the same spacing. The shear propagates the influence and the cloud forms first over the centre of the billows so as to extend the pattern : the condensation provides the energy for the overturning in the rolls and then billows proper are formed; the influence is then spread further into the clear air.

It seems to be a property of shear that it can propagate regularity of spacing once it has begun. If in one region regularly spaced clouds appear because there is some characteristic length in the defining conditions, such as spacing of heat sources or depth of layer overturned, then the spacing is propagated by a dynamical influence which induces further convection to assume the same spacing in places where there is no characteristic length. Though the depth of a cloud layer, or some similar depth, might be the origin of the spacing to begin with, in patches of cloud the spacing does not alter towards the edge, where the cloud is presumably thinner, and, as just mentioned, it is propagated into clear air.

The phenomenon of billows is not the same as that of cellular altocumulus where the cell size does seem to vary more readily, but there are cloud formations which cannot be obviously put into either category.

2.3 *Mammatus clouds*

These are clouds in which the motion is certainly downwards in the centre of the units. Masses of cloud sink into the clear air below and usually have a smooth rounded outline. The best examples occur in the environs of large cumulonimbus clouds where cloudy air may ascend rapidly and spread out over the top of drier air and then sinking of the whole mass occurs. The cloudy air then warms less rapidly and descends in drop-like masses into the clear air below. The clear air from below often ascends in a similar manner in between the descending masses and can produce very light channels in an otherwise very dark cloud mass. The life of each drop is of the order of 3 min and there is generally no regular pattern. The under side of stratocumulus-cumulogenitus displays the same phenomenon though less well developed.

Another mechanism for obtaining more liquid water in the lowest regions of a cloud than if the air that is directly beneath ascended into it, is for larger droplets to fall from

higher levels in the cloud. This has been suggested by Wagner (1948) as a mechanism for lowering the base of the cloud when the whole air mass subsides or for saturating and cooling the air immediately below cloud base by evaporation of the falling drops. If the precipitation continued it would then be contained in air that was denser than that immediately below it.

When a large area of precipitation is generated quickly, as for instance on the under side of an overhanging anvil-like part of a vigorous cumulonimbus, the outline of the cloud is often the lower boundary of a region containing precipitation. The base of this precipitation descends, and is usually well inclined to the horizontal, and the effective density of the air containing the precipitation is greater than that of the clear air. The base then generates a mammatus formation.

Drop-like formations also occur frequently in condensation trails. If the growth of the crystals in them is rapid enough to increase sufficiently the effective density of the air containing them before the trail is dispersed, the trail breaks up into pendulous lumps. This property of disintegration of a horizontal ' tube ' of fluid of different density from its surroundings can be demonstrated by introducing a fine jet, composed of water weighted and made visible by potassium permanganate or other dye, into a stream of water. The trail of ice crystals formed in a cold box in the laboratory along the trajectory of a pellet of ' dry ice ' through a fog of supercooled water droplets gathers into sinking lumps in the same way.

Regular geometrical patterns are not a characteristic of areas of mammatus cloud even though the size of the drops is restricted to a small range. One is led to consider whether their size is determined not by a length in the defining conditions but by the rate at which fluid is supplied to the drops. Rate of heating plays no part in the theory of convection cells and in the laboratory one is concerned to make it as small as possible while still effective. When the rate of supply of buoyant fluid is important we begin to formulate the concept of an environment through which it shall rise, and are led to consider the properties of these penetrating masses.

3. Theories of thermals

We are concerned in this section with ascending currents, usually called *thermals*, and how they are imagined to originate and behave. There is certainly no general theory of convection but ideas have grown up round special problems. The theory of *heated jets* expounded by Schmidt (1941) showed how the temperature anomaly, vertical velocity and width of the jet could, for an isolated point or line source of heat, be expressed as powers of the height above the source. The method of solution was derived from the study of boundary layers and turbulent wakes of cylinders and has likewise been applied to convection over a uniformly heated plane surface by Sutton (1948). Though he claimed observational support for his power-law solutions, and thought in terms of bubbles mixing with their surroundings, Sutton would deduce no physical picture of the processes of convection from his analysis, except, of course, that the scale of motion was also proportional to some power of the height.

The chief idea imported from the theory of jets is that mixing takes place between the jet and its surroundings, but the existence of the mixing is a hypothesis or is at least regarded as a property of fluids, and is not explained. The result of the mixing is to decrease the buoyancy by dilution and to increase the mass of ascending air, and the consequent steady increase in upward momentum is equal to the buoyancy and no accelerations can therefore occur. For this kind of flow to occur in the atmosphere a continuous intense solitary source of heat must be provided, and the eruption of Vesuvius

in 1944 provided a good example on calm days. Slater (1939) describes how several sailplanes soared in the cone of warm air (on a calm day) from the chimney of a cement works. Such instances are, however, exceptional.

Because sailplanes were often able to remain in an upcurrent for as much as 20 min, during which time they sank between 1,000 and 1,300 m through the air although the region in which they were able to ascend was often less than one third of this width, the thermals were imagined to be ascending columns of air, sometimes described as ' chimneys.' Sometimes it was thought that the air did accelerate upwards because the upcurrents were found to be weaker closer to the ground. Equally they were usually less broad low down and the widening could be ascribed to mixing, as in a jet. Thermals were imagined to be coming from some ' hot spot ' or part of the ground which was able to supply more heat to the air than the neighbourhood. This idea of thermals as 'chimneys' has predominantly, though not exclusively, found favour in America.

The pioneers of thermal soaring in Germany (see Pielsticker 1940) were probably more influenced by the remarkable smoothness of thermals. Since on many occasions the entry into a thermal was sudden they were likened to bubbles of warm air ascending, with no mixing of importance, up to the equilibrium level where they flattened out horizontally. The belief that mixing was unimportant is well exemplified by Höhndorf's claim (1947) to have seen once, under special circumstances, a bubble overshoot the equilibrium level, which was near the condensation level, and sink back below it again, making as many as eight complete oscillations before vanishing. It was well known that the lapse rate near the ground was superadiabatic and it was imagined that the relative uniformity of the earth's surface produced nearly horizontal isotherms so that though unstable the air was in equilibrium. If the equilibrium were disturbed by the slope of the ground, the presence of a ' hot spot,' or the sudden descent of a cold mass from above with a consequent scooping effect, a bubble of warm air from the superadiabatic layer where the isotherms were humped up would ascend and be replaced by colder air which would remain at the ground for some time before becoming hot enough to ascend. A ' hot spot ' or hump would therefore send up a series of bubbles, and many attempts have been made to measure the frequency with which they were released, but with no evident success. It seems that, if this picture be accepted, a chain of bubbles is released in quick succession followed by a long pause with no release.

The bubble model has found its most general acceptance in Europe, but its inadequacies are plain even to soaring pilots who make use of it because of the great variety of sizes and bumpy regions around them. They have been imagined to be elongated to various degrees according to experience of them.

Bubbles which mix into the surrounding air have been considered by Taylor (unpublished) and Sutton (1947). They considered spheres of very hot air but have rather assumed a symmetrical kind of mixing than tried to describe the mixing process, and have applied their idea chiefly to the hot mass of air generated by the atomic bomb. The appearance of the mushroom-shaped top of the cloud produced by it has suggested a kind of turbulent vortex ring or spherical vortex, and the idea of applying such a model to ordinary thermals has found adherents.

A theory, more simple because it offers a single unit of convection to explain all the phenomena, more complex because of the varied behaviour of the unit in combination with other units, has been proposed by Scorer and Ludlam (1953). Though hot spots play their part they are not an essential requirement, while the superadiabatic layer is not regarded as a clear-cut layer but as quite indefinite in extent. Their bubble model has a sharply defined upper cap at which mixing into the surroundings begins through the static instability there. Further down it merges almost imperceptibly into a wake

which is a mixture of the bubble with the surroundings through which it has passed. The bubble is rapidly wasted away and only ascends about twice its original diameter before it is completely mixed into its wake. The wake is then, for a short time, a region where ascent of further bubbles to greater heights is preferred. The amalgamation of wakes of small bubbles into larger bubbles goes on continuously as long as the supply is kept up. If the supply lapses a fresh growth must begin by the ascent of bubbles into unprepared air.

In this way the increase of the size of thermals with height is explained. The superadiabatic layer is the region of amalgamation of the smaller, less efficient, bubbles and wakes into larger ones; and when a larger one begins to ascend it is replaced at the ground by a mass of cold air from above. The convergence often observed at the ground towards the point of ascent of a bubble is of cold air to replace it, and this cold air does not itself follow the bubble up. At this point the wake plays an important role. It is a region of turbulent stresses, the stirring motion being due to the horizontal gradients of density, not of velocity in the first instance. These stresses nullify the minimum of pressure found immediately beneath a warm bubble, and so no organized horizontal convergence occurs below a bubble, except at the ground when it first leaves. Certainly no significant pressure minimum is to be expected beneath a cloud.

Because bubbles are rapidly wasted away when ascending along a path not recently travelled by other bubbles, most do not reach the equilibrium level for air ascending unmixed from the ground, but the height reached by thermals depends on the supply of heat to a region where agglomeration of bubbles is taking place.

4. Effects of wind shear

There is no documentation of observations which makes the testing of a theory possible. This is because the phenomenon of convection is so influenced by the nature of the locality (as described below) and the features of the weather special to the day, such as cloud amount, synoptic situation, time of year, condition of the ground and so on, that no such thing as typical convection occurs. Naturally, therefore, the theories have grown up around the experience of those formulating them and give a good qualitative explanation of what the authors each regard as the most significant observations. No theory is at present precise enough to make predictions of upcurrent strength or dimensions but some sort of prediction is possible in the case of thermals ascending in a shearing current.

Malkus (1949b), using the idea of the thermal ' chimney ' into which surrounding air was mixed, computed how the mixing into the jet caused it to move horizontally as it ascended into surroundings moving with a different velocity. The most important result was that the air of the thermal in a shearing airstream moves with a velocity measurably different from that of the surroundings. Hitherto relative horizontal motion had not been seriously considered. This result was taken into account in computing the slope of a cloud, but the observations are not accurate enough nor the slope of a cloud steady enough for any real test to be made of the theory, particularly because arbitrary assumptions about the level at which the updraught originated were also necessary. In principle the amount of mixing was deduced from observations on the ascent of the thermal, but in a rather roundabout way because the vertical velocities and amounts of dilution were unfortunately not measured for the clouds whose slope was observed. Scorer and Ludlam obtained a rough estimate of the ratio of the relative horizontal velocity to the vertical velocity, which again indicated that it should be measurable.

At the same time glider pilots have observed that often, when thermals are entered suddenly, an instantaneous increase in airspeed occurs. This has been rather dubiously explained in terms of the aerodynamic reactions of the glider on entering an upcurrent, but since no forward acceleration is sensed by the pilot it is reasonable to attribute it to

a genuine relative horizontal motion of the air of the thermal. According to Malkus's theory there is no reason why a decrease should not just as often be observed. Nor, according to a ' chimney ' theory in which mixing is important, is a thermal likely to be entered suddenly but would be bounded by a bumpy region.

In a shearing current clouds are observed to grow mainly on the up-shear side (which is usually the upwind side), and dissipate on the downwind side. Malkus explains this, in a manner not readily understood, as being due to the displacement of the upcurrent into the shear and thereby predicts the upcurrents to lie outside the cloud on the up-shear side. Such upcurrents have been observed by glider pilots. Clouds appear to grow into the shear by succeeding turrets ascending on the up-shear side. Since ordinarily shear is in the wind direction this may be due to the source of the thermals being fixed to the ground or at any rate to the lower layers of the air. The bubble theory explains this as due to the displacement in the down-shear direction of the top of the region of bubble building of which the cloud is evidence : succeeding bubbles ascending through the lower part of the region inevitably appear on the up-shear side.

Because of the relative horizontal velocity, the wakes slant down the direction of shear from the bubble cap, so that a glider, if it enters the thermal through the cap, as it must in order to do so suddenly, must enter from the up-shear side. At the same time the evaporation and the consequent downcurrents (described below) lie almost entirely on the down-shear side according to either theory, and this would compel almost all gliders to enter the thermal from the up-shear side, and thus observe an increase of airspeed, but this explanation does not apply well below cloud base where the phenomenon is most often observed, nor would the thermal be entered suddenly. Circulations within the thermal bubble could also affect the change in airspeed if the thermal were entered suddenly.

A rotation about a vertical axis would have the same effect if the thermal were entered obliquely against the rotation. If entered with the rotation no lift would be observed because of the decrease in airspeed and so the thermal would not be noticed as being entered suddenly. On the other hand a column rotating with a sudden decrease in moment outwards is an unstable configuration so that this explanation is inadequate.

The upcurrents in the clear air on the up-shear side are thought to be in the air rising above fresh bubbles ascending beneath. It is well known that pileus clouds are often formed in this way, and, as just mentioned, the fresh bubbles do appear on that side.

The bubble theory of Scorer and Ludlam stresses the importance of the cap of the bubble, for this is the only clearly identifiable part of the velocity and temperature fields that compose it. The rest of the field is deduced from it. The chimney idea emphasizes the properties of the flanks of the region of rising air. If this latter picture be accepted the air that is cooled (pileus cloud for instance) above a rising bubble might be observed to be simultaneously rising and colder than the surroundings and interpreted as a special kind of thermal so far admitted on no theory except in this way. This illustrates the danger of deducing from random traverses by aircraft a relationship between upcurrent and temperature anomaly. Because of the unknown velocity distribution in the wake similar paradoxes could appear there. The great value of the exploration by sailplane of a single thermal from top to bottom cannot be too much stressed.

5. Thermal sources

An upcurrent will persist only so long as fresh bubbles continue to enter it. A good thermal source is a piece of ground from which bubbles can ascend for longer without a break than in the neighbourhood.

One possibility is for warm air to be accumulated in among buildings, trees or standing ripe corn, to be released in large quantities when a gust of cold air sweeps across and

displaces the warm air. In the case of ripe corn or buildings the solid surfaces in contact with the air become a great deal hotter than green vegetation because none is used to evaporate moisture taken in by the roots. A large tree may transpire as much as three tons of water on a hot day.

Thermal sources may be places where the surface achieves a higher temperature in the sunshine. Besides green vegetation a surface of wet clay soil, which keeps cool by evaporation, may be a cool region whereas bare rock or sand achieves a higher temperature. The temperature achieved is not the only consideration for a surface that can continue to warm the air even when a shadow passes over it will provide a good source : concrete runways, areas of bare rock, or buildings and roads seem to do this. Though generally woods and forests are cool during most of the day, German soaring pilots report that air trapped among the trees produces thermals after the rest of the ground has begun to cool in the evening.

If a cool region lies downwind of a warm region thermals appear to be released at the downwind edge of the warm area. The idea is that the sharp horizontal gradient of temperature close to the ground causes the cool air to undercut the warm. Some pilots have reported finding thermals a mile or two out to sea when the general wind is offshore. When a shallow sea breeze is blowing these thermals are almost continuous and produce elongated cumulus clouds stretching out to sea from the highest points on the land. Other places where there is a temperature contrast at the surface, such as the edge of a wood or lake, or marked change in vegetation, are also said to be good sources.

The most powerful sources are undoubtedly provided by high ground. The heat is put in higher up and on slopes a horizontal temperature gradient is automatically produced. The first convection clouds of the day almost invariably appear over hills and anabatic winds feed them, and as we shall see in Section 6, convection is often prevented over the lower ground by sinking motion, so that throughout the day clouds may appear only over the mountains. Over the mountains of Spain large cumulus clouds tower upwards, covering large areas of the sky with their anvils, while almost no cumulus appear over the plains and valleys. Langmuir (1950) describes the same phenomenon in Honduras where the clouds over the mountains ' pump cirrus ' into the sky.

Davies (1951) has described the almost continuous ascent of air over a ridge in Derbyshire. On that occasion the general wind was light and along the ridge, while near the ground the drift was towards the ridge. The lower layers were heated by small bubbles and then moved anabatically up the slope.

It is often found that over the lowlands around York thermals are weak and cannot support a glider while the jurassic limestone and chalk escarpments of east and south-east England provide good upcurrents. This has been ascribed (Douglas 1943) to the nature of the soil, clay and carboniferous types respectively, but is more probably due to the latter being outcrop escarpments and good thermal sources for that reason.

Nothing of value is known about the temperature distribution over the sea when convection is taking place, but the temperature is unlikely to be appreciably reduced and is rather maintained by stirring when cool patches of air descend to the surface. Circulations within the sea make its thermal capacity effectively infinite, and thermals will grow in competition with each other, the width and spacing of them being determined by the amount of heat being transported upwards.

6. Types of motion

6.1 *Streets*

Clouds are often found arranged in rows called *cloud streets*. According to Welch (1953) dry thermals behave similarly. This has been explained as cellular motion arranged

as longitudinal rolls by the shear, and although the analogy is difficult to explain, Scorer (1952c) has suggested that as in other cellular motions an orderly arrangement may only occur when the heating is only just sufficient to maintain the convection. Cumulus clouds arranged in a rectangular pattern over wide plains or over the sea are sometimes seen, and the same condition is probably present. Isolated streets are often seen lying downwind from good thermal sources or along feeble fronts when rain is falling from them. Because thermals grow into shear, if any exists, streets will not lie exactly down the wind direction at the surface.

6.2 *Rotation of thermals*

If the surface contains no good thermal sources as, for instance, when it has a very small thermal capacity or is almost uniform, or both, like a sandy desert, the supply of thermals to the same volume is not kept up and no larger bubbles can be created. Under these circumstances the lapse rate increases to that appropriate to the transport of heat by small bubbles only and becomes superadiabatic up to 3-4,000 ft (Flower 1936), or perhaps higher. If rotation about a vertical axis can be produced the air may become organized into a *dust devil*. The necessary spin is created where the surface wind is retarded by the huts of a village or the trees of an oasis or at one end of a small ridge or embankment across the wind, and, as many writers have pointed out, is intensified by convergence. Flower infers that the lapse rate must be superadiabatic up to at least as high as dust devils are seen to ascend, and Swinn (1953) has soared to 7,200 ft in one in Egypt.

The rotation enables the rotating mass to ascend without disintegration. Rayleigh (1916) pointed out that if the moment of the momentum about the axis increases outwards the variable centrifugal force has the same effect as a stable stratification of density, and so mixing with the surroundings or wasting by mixing as with a bubble would be inhibited. Any mixing would tend to reduce the spin in the middle in such a way as to produce a stable configuration. This could equally well be caused by friction at the ground when the spin is produced by convergence of the air set in rotation by a mechanism such as that just suggested, and it may be noted in this connection that the air that is rotating contains dust drawn up from the ground. The friction at the ground therefore seems to be essential to the production of this kind of thermal.

If the convergence continued right to the centre with no friction the spin would become infinite. Or, if the column were drawn out vertically through being surrounded at all levels by denser air, the spin would be greatly increased and the pressure enormously reduced in the centre. In many, and possibly in all dust devils, the central core consists of air drawn at the top into this low-pressure region. Since the free air is not rotating, as is the air coming up from the layer influenced by non-uniform surface roughness, it can enter the core without acquiring an enormous spin. The air is unstable at all levels for downward as well as for upward displacements and so the core is usually colder and descending. If the supply of rotating air at the ground is cut off the truncated column often continues to ascend to a height of several thousand feet. Dust devils have been described also by Ives (1947). Swinn has made some remarkable flights in them by glider.

Moving dust devils usually follow a path curved to the right or left according as their rotation is anticlockwise or clockwise when viewed from above. This may be because the air most retarded by friction at the surface is on the side where the velocity is greatest, and this air feeds the more central part because it has less vorticity and will therefore be stably situated if surrounded by the rest of the air that ascends.

As would be expected the earth's rotation appears to have no effect on the direction of rotation.

Waterspouts do not originate at the surface but generally within a vigorous cloud. The pressure minimum beneath a warm bubble usually extends only a small distance below it because of the stresses in the disturbed motion of the wake. As we shall see below, the lapse rate in the air above cloud base usually exceeds the wet adiabatic, and to an extent depending partly upon the rate of heat transport. In regions where convection is extremely vigorous, as over the ocean in a polar-air depression, the lapse rate may be such as to form large bubbles very rapidly. There are no turbulent stresses until the bubble has ascended and undergone some mixing and so if the air beneath it possesses some rotation, as it well may in among vigorous up and down motions present in neighbouring thermals, it will converge into the low-pressure region below the newly-formed bubble with very little stirring. In this way a rapid rotation and a very low central pressure are induced, and again, since the cause of the low pressure is not a bubble which has ascended through the air, the air further down is subjected to similar convergence and the spout extends down to the surface. Whether the cloudy air in the centre actually originates in the cloud or not is not known. At the sea surface a copious spray is produced, probably as the wind blows over waves produced by the rapid fluctuations of pressure as the spout moves.

Tornadoes are a similar phenomenon but are much less common because the land does not usually supply heat rapidly enough over a large region for the lapse rate above cloud base to be sufficiently steep for the very rapid creation there of large bubbles. The air that converges under large bubbles leaving the ground is necessarily colder and from above the bubble base and remains at the ground, while the bubble ascends and forms a turbulent wake. Rotation does not therefore usually occur under ordinary thermals at any stage.

Koschmieder (1951) has described the formation of funnel clouds over an intense heat source provided by a stream of lava.

Tropical hurricanes, about which there is a considerable literature, are mentioned here only to remark that, in common with dust devils, they appear to develop where the air is positively unstable, not simply not stable, over a considerable depth, in this case for saturated air, and that descending motion occurs in the middle. The air in the eye is, however, warmer, not colder than the surrounding air, perhaps because it has descended from the stratosphere. According to Bergeron (1949) hurricanes die out in passing over land not because of the convergence made possible by friction but because the heat source, the sea, on which its continuance depends has been removed.

6.3 *Downcurrents and sonic disturbances*

No theory so far described makes any definite prediction about where the downcurrents occur to compensate for the thermals. Isolated jets are presumed to produce no down-motion except at infinity, though some might be expected if many heat sources were distributed over a very wide (infinite) area. The only limit to the distance from the thermal at which the down-motion can occur is placed by the maximum atmospheric signal velocity. This is equal to the velocity of sound at some level in the atmosphere, and a thermal of duration 20 min cannot be compensated for by downcurrents more than 360 km away, but if a region of convection lasts several hours the distance of the sinking motion may be many times this. Koschmieder (1940) says that the sinking occurs at a great distance and, as we shall see, this may often be a condition to be fulfilled before convection can persist while at other times it is the result of the convection and enables it to continue.

The expansion which results from air being heated is made possible (Scorer 1952a) by the outward movement of the surrounding air as a sonic disturbance passes outward from the heated region, and the result is that for practical purposes the density of the heated air is immediately lowered and the pressure beneath it falls as a result. The air above is lifted initially but pressure gradients at high levels are smoothed out by a sonic disturbance of a type similar to the Krakatoa air wave or the semi-diurnal pressure wave. It has been argued (e.g., Bleeker and Andre 1950, Sawyer 1949) that when a volume of air is heated the isobaric surfaces above are lifted and so an outflow at high levels occurs; the pressure is then said to fall at the surface and an inflow begins there, and because this inflow is impeded by friction at the surface the pressure deficit is not made up and a region of low surface pressure results. Bleeker (1950) even asserts that if it were not for friction all pressure gradients at the surface would be greatly reduced by inflow there. The origin of this error is not clear. It is in contradiction to the observed filling up of depressions as they move over land where, it could equally be argued, friction makes flow inwards across the isobars possible. Sutcliffe and Forsdyke (1950, p. 202) have even suggested that pressure gradients are usually less at the surface than higher up because of the influence of the boundary, probably through friction.

Since the outflow from a heated region is by means of a sonic disturbance which results from an external source of energy the compensating rise in pressure may be distributed anywhere a sonic disturbance can reach, and with the speed of sound. After the pressure has thereby been re-distributed the convective motion begins. It is of a different type because it results from the operation of a conservative field of force and converts potential energy into kinetic. The sonic disturbance results from the application of an impulse to the air by the increase in pressure that results when air is heated without expansion and the pulse travels through the air and produces a slight displacement.

In view of what follows it seems to us unprofitable to discuss the downcurrents apart from the motion on a much larger scale than the individual thermals. To understand this it is necessary to describe the mechanism whereby convection warms the air. In cloudless air a thermal must either mix with its surroundings or spread out at its equilibrium level, and in either case only the regions reached by the thermal air itself are warmed. The sinking motion in the surroundings produces no local warming because the air is not stably stratified – otherwise there would be no considerable convection. In order to warm a layer over ground that is more or less uniformly heated, thermals must ascend more or less everywhere and infuse their heat into the whole mass. Above the condensation level heat is temporarily borrowed which enables the ascent of the thermals through a stably stratified environment. The sinking motion therein warms it. Thus it is possible for a whole mass of air to be warmed by convection clouds occurring in only a small part of it. It is seen below that the actual air of the cloud thermals does not diffuse heat into the surroundings by mixing.

If clouds occur, therefore, since the air between them is warmed more efficiently than the air into which they ascend they will continue to ascend in the same place. Cumulus clouds situated throughout the day only over the higher mountains of a country such as Spain provide a mechanism for warming the whole mass of air above the condensation level, below which the heating is achieved mainly by widespread dry thermals. This idea has been confirmed by the experiences of glider pilots (Scorer 1952b). The adiabatic layer is fed towards the cloudy regions by anabatic winds.

The circulations set up in this way must have some inertia and probably overshoot the equilibrium state when convection ceases in the evening. The subsequent swing back in the opposite direction may be a dominant cause of the reversed circulations observed at night, though katabatic winds play their part. Clearly a quantitative study of

this process is desirable. The convection of a day seems to have a considerable influence on the convection of the succeeding night, while the day seems to be relatively unaffected by the convection of the preceding night (Scorer 1952b). This is understandable if it is the inertia of the daytime circulation that is largely responsible for the nocturnal circulation in the opposite direction : the oscillation being damped shows little tendency to persist into the next day, and it is not renovated by thermals from the surface at night.

Because of the possibility of warming the whole air mass by means of a few clouds, streets of clouds can continue to warm the whole mass, whereas streets of clear-air thermals are probably less persistent.

We return therefore to envisaging a circulation on a scale larger than the individual convection clouds which resembles the cellular convection which we have discarded as an interpretation of the thermals themselves. In the case of cloud-street formations the cell size has been reduced almost to the size of the average cloud bubble. If a larger scale circulation exists, the convection in the region of convergence and general ascent is more vigorous than cloud-street convection.

Some attempts have been made to fit all other forms of convection to the cellular model. Durst (1932) drew an analogy with the ' horseshoe ' pattern assumed by cells in shallow liquid over the top surface of which a wind blows. The columns in the centres of the cells are pushed over to one side at the top surface. The sudden gusts of cold air observed as thermals pass were identified with the edges of the cells which now have almost a discontinuity at the points to which their centres have been pushed. Albrecht (1942) presents a somewhat similar picture, based however on a bubble concept, of thermals being released by the scooping effect of a micro-cold front. This effect undoubtedy occurs at the downdraughts produced by cumulonimbus (Suckstorff 1938), but in the absence of precipitation it seems unlikely that the cold gusts originate higher up than just above the adiabatic layer. They are a necessary part of the process of the beginning of ascent of a large bubble, rather than the cause of it. Just as the most important feature of a bubble is the discontinuity of the temperature at its cap, so the lower surface of a descending cold bubble is well defined and a sudden arrival of cold air at the ground occurs. The generation of these discontinuities within a previously homogeneous fluid has not been properly explained. The equations of motion are non-linear, and the explanation of the progressive accentuation of gradients is bound to present difficulties.

6.4 *Sea breezes*

Using relaxation methods at each time step of a progressive integration of the equations for the case of air originally stagnant across a coastline when the land is heated, Pearce (1952) achieved some success in demonstrating the production of a front at the surface. Such fronts do occur (e.g., Peters 1938) but have not been satisfactorily explained in any published treatment of the sea-breeze problem. Some simplification is necessary because all factors together produce a problem of intolerable complexity. The difference in mechanism of heat transfer over the land and sea, the difference in stability (there is no adiabatic layer over the sea), the ' Eulerian ' acceleration, the earth's rotation, any already existing drift of air and the transfer of momentum by thermals – these are the chief sources of difficulty. In simplifying the mathematics by assuming a solution with a daily period an unreal difficulty is encountered, for infinite terms appear in the equations at 30° latitude when the earth's rotation is allowed for. Because the sea breezes there are not catastrophic the assumption of interminable repetition must be abandoned and the problem complicated in consequence.

It cannot be said that any treatment of sea breezes is satisfactory. Most are considered to be commendable if they reproduce the well-known characteristics of actual sea breezes

which have long been explained in the most simple terms, and such results are by no means the monopoly of any particular model. Perhaps because it is thought easy to explain sea breezes they have scarcely been accorded the attention they deserve, for almost all features of convection that we have described can play their part to varying degrees in their establishment.

7. Cumulus clouds

In small thermals which barely reach the condensation level ragged *fractocumulus* forms, with no clearly defined base. From the ground larger clouds appear to have well-defined level bases at a uniform height, but from close at hand it may be difficult to define the level of the cloud base to within a hundred feet, and variations considerably greater than this are sometimes found within a few miles. According to glider pilots the base of a large cloud may be a few hundred feet higher than the general level, over a limited area where a strong updraught occurs, so that this part of the cloud base is raised in a dome-like impression (Scorer and Ludlam 1952). The glider pilot ascending in this hollow can see the ground beneath some time after the horizons have been obscured. This might be due merely to the differing lengths along the lines of sight which are within cloud when the pilot is just above a uniform base, but this explanation does not usually satisfy those who observe the phenomenon. At other times the base is found to be lower where the updraught is greatest.

Above the level of the cloud base the lapse rate in the clear air becomes less than the dry adiabatic, but usually is at first considerably greater than the wet adiabatic, so that the temperature excess of the thermal bubbles inside the clouds, and their buoyancy, may increase. The buoyancy of the cloud air is proportional to the excess of virtual temperature, less a correction to take into account the weight of the condensed water. At temperatures below — 10°C the virtual temperature is practically the actual temperature, but with a constant clear-air humidity the difference increases until at 10°C it may often be 1°C greater than the actual temperature. The buoyancy due to an excess of 1°C, which is the order of that usually found, is just nullified by a condensed-water concentration of about 3·5 g/kg of air, a value which may commonly be exceeded inside large clouds. Both corrections to the excess of actual temperature must therefore be taken into account in estimating the buoyancy of cloud air, particularly at the higher temperatures.

7.1 *The adiabatic rise of cloud air*

The *parcel theory* of cumulus growth, due mainly to Refsdal (1930) and Normand (1946) assumes that the cloud air rises through an undisturbed clear-air environment, so that on an indicator diagram its temperature at any level lies on the saturated adiabatic which passes through the point representing the cloud base (Fig. 1). If the condition of the clear air as revealed by a sounding is plotted on the diagram, the excess of virtual temperature and the buoyancy of the cloud air at any level can readily be determined.

According to the theory the cloud air accelerates upwards while the buoyancy is positive, attaining a maximum speed at the *equilibrium level* (Fig. 1) and then decelerating under a negative buoyancy until its kinetic energy is expended. In practice it has become customary to expect the ascent to cease a little distance above the equilibrium level, but even so forecasters often noticed that cumulus tops were limited to much below this level, and felt the need for an improved theory which would give a more reliable estimate of the size attained by the clouds.

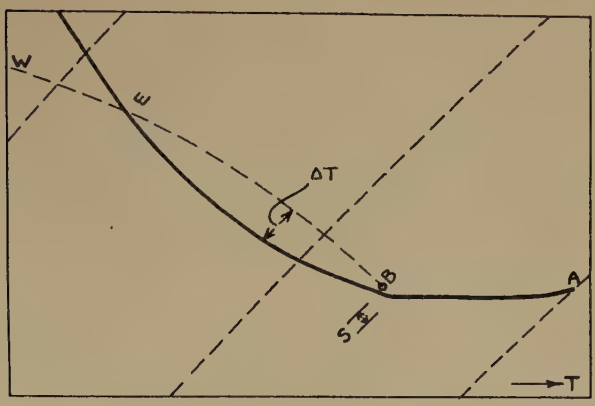

Figure 1. Typical features of the state of the atmosphere during cumulus convection, as shown on a tephigram. The thick line shows a sounding, with a superadiabatic lapse rate near the ground (A). The point B represents conditions at the cloud base, and the pecked line BW is a wet-adiabatic intersecting the sounding at E, the 'equilibrium level.' The maximum possible temperature excess ΔT inside the clouds is that between the sounding and the wet-adiabatic. In a shallow layer S just below the level of the cloud base the lapse rate may be noticeably less than the dry adiabatic. This layer has been called the 'sub-cloud layer.'

An attempt to provide one was made by Petterssen, who, following Bjerknes (1938), supposed that warming of the clear air between the cloudy updraughts, due to a compensatory settling, diminished their temperature excess and the energy liberated inside the clouds. He suggested (Petterssen *et al.* 1945) that the cloud tops were to be expected only a little above the level at which the lapse rate in the clear air first becomes equal to the saturated adiabatic (Fig. 1). The reasoning behind this suggestion is not clear, for the effect he considers only diminishes the buoyancy of the cloud air, and near the cloud tops the fraction of the sky occupied by ascending clouds becomes so small that the simple parcel theory should be equally applicable. The statistics presented in support of this suggestion are not convincing, although they confirm that the parcel theory often greatly overestimates the cloud growth.

The equation of continuity, which these writers use to demonstrate the necessity for compensating sink between the cloud updraughts, does not show where or over what extent the sink will occur. Several writers have suggested that it may be distributed over such a wide area, extending beyond the region of convection, that its effects are inappreciable. Our subsequent discussion shows that it is not profitable to consider it apart from other large-scale vertical motions.

The essential feature of the cumulus convection which these simple theories overlook is the continuous mixing and evaporation of cloud into clear air. Recently studies of cumulus growth have been concentrated upon the effects of this mixing.

7.2 *The effect of mixing on cumulus structure*

At the edges of a cumulus the mixing of the cloud into the clear air is accompanied by the evaporation of cloud particles and a chilling whose magnitude depends upon the quantity of water evaporated, the humidity of the clear air and the initial temperature excess of the cloud air. With the clear-air humidities ordinarily encountered (between 50 per cent and 80 per cent) and a temperature excess of 1°C, the evaporation of liquid-water concentrations exceeding $\frac{1}{2}$ g/kg of air involves the mixing of the cloud air with about 0·5 to 5 times its volume of clear air, and causes a chilling of between 1°C and 2°C. This chilling is usually sufficient to produce a negative buoyancy, and consequently

the details on the flanks of cumulus are seen to be sinking, although the cloud as a whole may be growing.

Well inside the cloud it may be expected that the mixing and its effects are much less pronounced : the buoyancy is reduced little, if at all, and the cloud grows in pyramidal shape by the protrusion of its interior. In this way the more protected internal parts of the cloud are soon brought to its edges and they, too, then suffer rapid mixing with the clear air. The cloud is continuously turned inside out and exposed to evaporation, and the size which it attains must depend upon the rate at which fresh thermals enter its base to overcome this wastage.

Even on days when very large clouds grow, the great majority of the cumulus reach only a small size and have a life of only several minutes. Evidently they represent the rise of isolated thermals; they are clear illustrations of the inadequacy of former theories of cumulus growth, none of which suggests that the clouds will not all grow to the same size.

Stommel (1947) drew attention to observations of trade-wind cumulus which showed that their tops are generally much below the level which cloud air could reach if it rose adiabatically from the cloud base. He assumed that by some mechanism clear air was incorporated or ' entrained ' into the cloud updraught, causing the evaporation of some of the condensed water and a substantial reduction of the temperature excess. Subsequently a number of American writers have discussed the effects of such an ' entrainment '; their work has been reviewed by Malkus (1952). Apart from emphasizing the importance of mixing processes during cumulus growth, the theories of ' entrainment ' have not so far been useful, for they have been based on the concept of the uniform mixing of arbitrary proportions of clear air into the cloud updraught, and the bare hypothesis that mixing occurs has led to no explanation of how it is caused.

7.3 *The structure of large cumulus*

It seems essential to construct some physical model of the mixing of clear air into clouds. Scorer and Ludlam (1953) have attempted this. They observe that large cumulus clouds exhibit a succession of prominences having a characteristic diameter of about a kilometre and identify these with individual bubbles which ascend through the cloud base as thermals, or which may be assembled within the cloud. They propose that in general such bubbles have a maximum diameter of rather less than 2 km, and that large cumulus are composed of a number of bubbles and of their wakes, in which there is a vigorous mixing with the surroundings (clear air or other wakes).

Small clouds less than about 2 km deep contain only one or two thermal bubbles. A large volume of clear air is mixed into the wakes of these bubbles, and the rate at which the cloud turns inside out is such that the inner parts can rise a distance only about equal to the horizontal extent of the cloud before being halted by exposure to mixing with the clear air. A large cloud can be built up only by the rise of numbers of thermals in close succession, and must normally have roughly the same pyramidal shape. Those thermal bubbles which rise near the edges of the cloud mass suffer the most severely from mixing with the clear air and are halted the soonest. The wakes of these bubbles are mixed into the wakes of bubbles which rise nearer the middle of the cloud : here the dilution with clear air is less, and the bubbles are able to ascend farther, eventually emerging to form the cloud tops, but in this way some of the clear air is mixed even into the middle of the cloud mass.

As the ascent of thermals occurs irregularly the large cloud has a chaotic structure. It may consist largely of bubble wakes, in which the temperature may be lower than that

of the outside air and the concentration of condensed water only a small fraction of the value corresponding to adiabatic ascent from the cloud base. The buoyancy of large volumes may be negative, so that downdraughts occur; but these can produce only small vertical displacements of air which are certainly not to below cloud base unless falling precipitation maintains a chilling. In this case the downdraught can be maintained, and often in thundery weather extends down to the ground, producing squalls and other phenomena which have been described in detail by Suckstorff (1938) and Byers and Braham (1949).

This latest picture of the structure of the large cloud accounts for the observations made inside them by powered aircraft, and reconciles these with rather different impressions gained by glider pilots.

The glider pilot has often emphasized the ' smoothness ' of the air flow which he finds while being lifted rapidly in powerful cloud updraughts, and that ' bumpy ' conditions are characteristic only of the edges of updraughts (see, e.g., Wichmann 1948). He has been inclined to discount the idea of updraught air being mixed with clear air in any degree, since to be effective over the width of an updraught during its lifetime (over perhaps a kilometre within 15 min) it would be associated with turbulence on a scale to which his aircraft is sensitive. However, he takes measures to stay in the interior of rising bubbles, and it is understandable that he should find the air ' bumpy ' only when he leaves the updraught or when it begins to fail.

On the other hand the pilot of the powered aircraft makes a rapid horizontal traverse of the cloud. He passes through chaotic bubble wakes and perhaps one or two bubble centres, experiencing a bumpy succession of variable up- and down-draughts.

The glider is well suited to exploring those parts of the large clouds, the bubble centres, which are of greatest interest. Within these bubbles the condensed-water concentration and the updraught attain their maximum values, so that they are the seats of rain and hail formation, severe aircraft icing and thunderstorm-electricity generation. The powered aircraft encounters these regions only by chance, and then passes through them in a few seconds at a speed which makes the recognition of hydrometeors and the measurement of liquid-water concentration and temperature difficult and uncertain. The measurements made in such flights must be interpreted with great care; it must be considered that mostly they refer to conditions in bubble wakes, and that no definite relations are likely to exist between the details found on successive traverses of a cloud separated by intervals of several minutes.

Accurate observations of temperatures are particularly difficult to make at high airspeeds because of the uncertain correction for kinetic heating of the thermometer element in cloudy air. When the conventional dry-adiabatic correction appropriate in clear air is made to the indicated thermometer readings, cloud temperatures are usually deduced to be lower than the clear-air temperatures at the same levels, so that the cloud air appears to have a negative buoyancy. Almost certainly, however, the reduction of the indicated temperatures should be less by an amount depending upon the droplet concentration in the air, for as the droplets approach the thermometer bulb they are not subject to an adiabatic warming; rather, they wet the thermometer element and there is some evaporation. Errors of two or three degrees Centigrade might in this way enter the deduced temperatures. However, the observations made by the aircraft of the Thunderstorm Project (Byers and Braham 1949) showed that the air inside growing cumulus possessed a positive temperature excess of up to about 4°C, while negative temperature excesses of about the same amount occurred in the downdraughts of mature thunderclouds. There was a clear relation between the temperature excess and the draught speed, the high excesses being associated with average speeds of about 10 m/sec.

Recently Byers (1952) has published the detailed analysis of one of these thundercloud traverses, in which he states that a moist-adiabatic correction for kinetic heating at the thermometer element has proved to be the proper one inside ' wet ' clouds, although it is not wholly reliable. This implies that the element is wet and indicates the wet-bulb temperature of the air passing over it. In the example he describes the dry-adiabatic correction led to the temperatures observed at 20,000 ft inside a thunderstorm appearing rather lower than those outside, but in fact the region of strong updraught (about 12 m/sec) was associated with a temperature excess of 5°C.

Warner and Newnham (1952) have made continuous recordings of the liquid-water content, during horizontal traverses of small cumulus clouds, which well illustrate its irregular variation within the cloud. The peak values which they measured were less than $\frac{1}{2}$ g/m^3, less than a quarter of the values corresponding to the adiabatic ascent of air from the cloud base. No reliable measurements of liquid-water concentration within large clouds have been published. Values have been inferred from the rate of accretion of cloud droplets upon riming cylinders and discs, but these are mostly averages across the whole extent of clouds, which must inevitably be much smaller than the maximum local concentrations likely to occur in the bubble centres. Moreover, Ludlam (1951) has shown theoretically, and Fraser, Rush and Baxter (1952) have confirmed experimentally, that this method is incapable of measuring high concentrations at temperatures not far below 0°C.

The observations made during the Thunderstorm Project contain an unrivalled series of measurements of updraught speeds in thunderstorms. Maximum speeds of about 10 to 25 m/sec were observed at heights of 10,000 ft to 25,000 ft, in clouds which mostly reached above 35,000 ft and occasionally reached 50,000 ft. The most frequently encountered width of updraught was about 5,000 ft, which supports our estimate of rather less than 2 km for the characteristic width of individual bubbles.

It is interesting that the radar examination of these storm clouds indicated that a particular ' cell ' giving a well-defined radar echo was a large cloud containing a number of smaller active units called ' turrets,' and which represent individual columns or bubbles. Thus it was observed that the highest echoes were built in steps, in which successive turrets extended above the top of the first-formed column of echo. The second turret usually extended about 5,000 ft above the first, which is about the additional penetration into clear air which would be expected if a bubble of about this width emerged relatively undiluted from the previous cloud tops.

The measurements so far made inside large clouds do not provide reliable information on the important question of how much dilution is suffered by the bubbles which rise through their interiors. However, the fact that the lapse rate in the clear air outside large clouds is often little different from the wet adiabatic suggests that in these circumstances there can be very little dilution. Espy, Klein and Palladino (1945) developed a fairly successful method of forecasting the level of cumulus tops in polar air outbreaks over the northern North Atlantic; this method consisted of predicting the temperature at the cloud base (from the sea-surface temperature and an assumed surface humidity), and finding the point of intersection of the wet adiabatic through the point thus defined on an aerological diagram with a curve representing the initial temperature distribution in the unmodified air mass, adjusted to allow for a mean vertical motion. The cloud tops were forecast to be 750 ft higher, and a test using aircraft cloud observations showed the prediction to be in error by 3,000 ft on only one occasion out of forty. This work is a straightforward application of the parcel theory, and its success indicates that no significant dilution of bubbles occurs inside these clouds, since Scorer and Ludlam (1953) have shown that the bubbles cannot be expected to ascend much above the equilibrium level at which their buoyancy becomes zero.

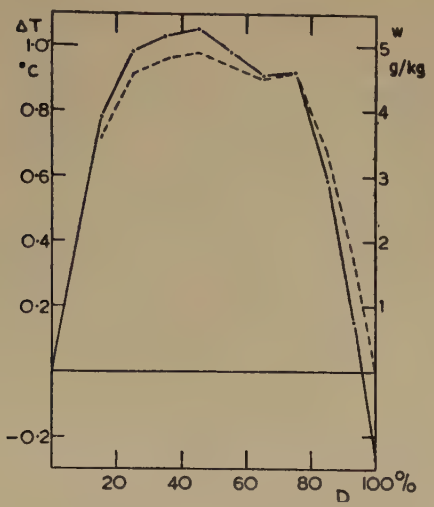

Figure 2. The average maximum temperature excess ΔT at various fractions D of the cloud depth, for 77 observations of cumulus more than 200 mb deep, made over the northern North Atlantic during Oct.-Mar. 1943-4 and Oct. 1944. The pecked line shows the liquid-water concentration w which could be supported by the buoyancy due to the excess temperature, assuming that this already at the cloud base amounted to 0·27°C.

Some support for this view is provided by an examination of the cumulus observations and simultaneous soundings made over the northern North Atlantic by British meteorological-flight aircraft during the war. When large clouds were observed (of depth greater than about 7,000 ft), their tops were almost always very close to the equilibrium level of the parcel theory, provided that it was determined by wet adiabatics calculated with respect to liquid water even at temperatures below 0°C. The cloud tops were usually well above the level indicated by Petterssen's theory, namely where the lapse rate first becomes equal to the wet adiabatic. This is illustrated in Fig. 2. It was obtained from an analysis of 77 soundings made during the months of October-March 1943-4 and October 1944, and shows the average difference between the temperature observed in the clear air, and that corresponding to adiabatic ascent from the cloud base, as a function of the cloud height. The average temperature at the cloud base was about 5°C, and the values of actual temperature difference can be regarded as differences of virtual temperature. It will be seen that the virtual temperature excess increases upwards from the cloud base to reach a maximum average value of about 1°C nearly half-way to the cloud tops, and then suddenly decreases near the tops to become − 0·27°C at the level of the tops.* On only eight of the soundings did it anywhere reach 3°C. In general the virtual temperature excess is sufficient to give a positive buoyancy to cloud air containing the maximum possible condensed-water content, except within some $\frac{1}{2}$ km to 1 km of the cloud top. This suggests that the bubbles which compose the cloud tops are protected from considerable mixing within the cloud, but suffer rapid dilution and evaporation on emerging near the summit level. It must be emphasized that this conclusion is valid only for the largest clouds of all those present, and only when they are more than about 7,000 ft deep. The observations were not sufficiently detailed to warrant a similar discussion when the clouds were smaller, but it was clear that on many such occasions their tops were considerably below the equilibrium level of the parcel theory, and this of course must also have been true for the majority of the cumulus present on the days for which the soundings were analysed.

* This suggests that inside the cloud bases the virtual temperature was about 0·27°C higher than that observed in the clear air at their level.

7.4 *The condition of the cloud environment*

Cumulus over the sea in polar air masses is irregularly but fairly evenly distributed, and the convection may be regarded as an efficient agency for stirring and mixing the air. Air which at one moment is clear contains a proportion of more or less recently evaporated cloud, and eventually may itself be again involved in cloud formation or growth. The condition of the clear-air environment may therefore be considered an instantaneous property of the convection process, as may the condition of nearly constant potential temperature and specific humidity which is set up in the air beneath the cloud base. We may regard this rapid adjustment of the lapse rate to the convection as a consequence of the short life (20 min or less) of the individual thermals and clouds.

Below the cloud base the mixing of thermals into their environment warms this air, while vertical motions produce no local change of temperature except near the ground and the top of the adiabatic layer. The mixing therefore makes the principal contribution to the warming of this layer. Above the cloud base the thermal bubbles may become considerably warmer than the clear air, but the mixing in general chills, and after the evaporation of cloud the mixed air must sink. Because it then warms at the dry adiabatic rate it does not settle far in the layer of near-saturated-adiabatic lapse rate before reaching equilibrium with the outside air. In the cloud layer, therefore, the mixing processes produce no local heating, and the warming of this layer can be regarded as due to widespread settling motions which occur between the cloud updraughts in the manner already indicated. Superimposed upon and inseparable from these motions are the widespread vertical movements due to disturbances on a synoptic scale. These therefore exert a controlling influence upon the convection. They are most pronounced in the middle troposphere, and hence the upward movements associated with low-level convergence, perhaps caused solely by the warming due to the convection itself as previously discussed, tend to exaggerate and maintain the convection, while widespread subsidence tends to suppress it. The existence of general upward motion is illustrated by the increase in cloudiness and the small rate of warming often observed during vigorous convection. If throughout an hour the total quantity of condensed water present in the clouds is unchanged, and if 1/10 of the sky contained updraughts of 3 m/sec, a general compensating sink of 1/3 m/sec would produce a warming of the air above cloud base of several degrees Centigrade. Such a rapid warming is not observed; if it did occur the convection would quickly be diminished.

Bunker *et al.* (1949) made a number of detailed aircraft soundings amongst the trade-wind cumulus over the Caribbean. They found a layer of practically constant potential temperature and specific humidity which extended from near the sea surface to within a few hundred feet of the level of the cloud base. Above this level the lapse rate becomes less than the dry-adiabatic and the specific humidity decreases, and over several hundred feet the lapse rate may be very small or zero. These writers therefore described it as the ' stable layer,' or the ' sub-cloud layer,' and interpreted it as an indication that there were no thermal updraughts below the cloud bases. However, it was difficult to find in relatively cloudy areas, being well marked only in large clear spaces ten or twenty miles across. It therefore seems likely to be produced by settling motions outside the cloud clusters, which cause the potentially warmer and drier clear air at cloud levels to sink some distance into the adiabatic layer before becoming incorporated into it by mixing. This settling may well be less marked within the comparatively cloudy areas, and the ' stable layer ' can be regarded as a normal feature of the temperature distribution well away from concentrations of thermals or clouds, particularly when the air mass is subject to general subsidence. Under these conditions it is not peculiar to the trade-wind regions, but also may be found during convection caused by sunshine overland, when

there is no doubt that the clouds are formed by the ascent of thermals from the surface layers (Ludlam 1953).

The oceanic cumulus, observed by Bunker *et al.*, were assembled into clusters separated by large clear areas almost free of cloud. The reason for this grouping is not clearly understood, but evidently the convection in the cloudy patches is sufficient to warm the whole air mass at the rate determined by the change in surface temperature and the general convergence. In the cloudy patches the condition of the air will be that appropriate to the convection; in the clear areas the air is warmed steadily by subsidence. The cloudy areas probably migrate through the air mass under the influence of the wind shear, as Scorer and Ludlam (1953) suggested happens with individual clouds.

In general the fraction of the sky which is occupied by the cloud updraughts diminishes with height above the level of the cloud bases, because individual clouds taper upwards and the smaller clouds outnumber the larger. We may then expect the same rate of transfer of heat upwards to be effected by weaker updraughts near the cloud bases, associated with smaller temperature excesses, and therefore the clear air sounding on the aerological diagram is broadly a curve concave towards the wet adiabatic through the point representing the cloud base. Above the cloud-base level, in the region of lapse rate greater than the wet adiabatic, we may expect bubble agglomeration to occur within large clouds, just as there is thermal agglomeration in the dry air close to the ground where the lapse rate exceeds the dry adiabatic. The exact nature of the temperature and humidity variation throughout the convective layer is determined by the peculiar properties of the clouds and the distribution of general vertical motion.

7.5 *The control of convection by large-scale disturbances*

The maximum height to which convection can reach, in an air mass heated at the surface, is the level at which the air, if saturated at the same temperature, would possess the wet-bulb potential temperature of the air near the surface. It is therefore dependent upon the modification of the high-level temperature distribution by widespread vertical motion, and upon the temperature and humidity at the surface. Over a particular stretch of ocean the latter parameters vary little from average seasonal values, and therefore during convection in polar outbreaks of constantly recurring character the most important factor in determining the upper limit of the convective layer, that is the level of the cumulus tops, is likely to be the widespread vertical motion associated with synoptic-scale disturbances. In this connection it is interesting that Crutcher *et al.* (1950), forecasting the height of cumulus tops on the Washington-Bermuda air route, found a striking relation between the observed height and the vorticity of the streamlines at the 700-mb level in the vicinity of the observed clouds. The correlation coefficient varied from 0·73 to 0·89 when 454 observations were grouped by months of the year. The height of the cloud tops varied from about 7,000 ft with zero vorticity to over 20,000 ft with the more intense cyclonic vorticities, which presumably were associated with upward motions. Petterssen *et al.* (1945) also found a similar relation, the vorticity of the 750-mb flow being cyclonic in 94 per cent and absent in the remaining 6 per cent of the occasions in their analysed data when cumulus over Britain reached or surpassed the 400-mb level. Forecasters are of course well aware of the close association of strong convection and the synoptic situation.

Bryson (1951a) has pointed out that over the tropical oceans air-mass and sea-temperature variations are insignificant factors in determining the development of cumulus convection, which is controlled almost entirely by the synoptic situation. He devised a scale on which to represent the degree of cloud development, and used it to investigate the patterns of cloud development in different kinds of weather disturbances (1951b).

7.6 *The diurnal variation of convection*

Bryson also showed that over the open ocean in the west Pacific there is very little diurnal variation in cumulus activity; if anything, the nocturnal clouds were rather less well developed. This stands in marked contrast with the nocturnal maximum of thunderstorm frequency which occurs in many coastal waters and enclosed seas.

Over land masses, convection is almost always due to the heating of the ground by sunshine, and there is a marked diurnal variation. Above the level of the cloud base there are often only very small changes in the temperature distribution, but the clouds which form early in the day are small even though the parcel theory suggests that cloud air could ascend to great heights. This is because the small rate of heating and the plentiful distribution of thermal sources cause the thermals to be small, well-scattered and produced only intermittently at particular sources. As the heating progresses larger thermals are produced, and in more rapid succession over favourable sources, so that bigger clouds can be built. Hills which, as described previously, are especially prolific thermal sources, are of special importance in the production of large clouds. On the other hand over valleys persistent subsidence subdues or even suppresses the cumulus growth.

The clouds usually attain their maximum development about the time of maximum surface temperature, but occasionally when there is an inversion or stable layer not far above the cloud base the cloud bubbles reach it and spread out to form a stratocumulus-cumulogenitus which shades the ground and reduces the convection. On other occasions the level of the cloud tops changes little while the level of the cloud bases rises steadily with the progressive warming of the surface layers, so that the clouds shrink and may eventually disappear.

When the surface temperature has fallen one or two degrees below its maximum the superadiabatic layer near the ground disappears; there is now no source of air potentially warmer than that in the adiabatic layer, except locally where the surface temperature is maintained on slopes facing the sun and other favourable sites. The clouds therefore decrease in size and number and usually disappear considerably before sunset. Towards sunset the stabilization of the lower layers may cause the formation of wave motions near hills, sufficient to cause a renewed condensation at about the previous cloud-base level, where a lift of only several hundred feet is sufficient to bring the air to its dew point. The clouds thus formed are in an environment whose lapse rate is steeper than the moist adiabatic and, unlike the typical wave clouds, they break up into an irregular small-scale convective pattern and may drift away from the hills as patches of stratocumulus, which is often mistakenly described as cumulogenitus.

In the late evening the normal daytime pattern of mean upward air motion over hills and settling over valleys is reversed. The nocturnal upward movement of air over valleys, and over lakes and enclosed seas surrounded by land breezes, may lead to a renewed condensation over these regions and appears to be the cause of nocturnal maxima of thunderstorm frequency. These have been shown to occur on the coasts of the Great Lakes and of Lake Victoria, for example, and Neumann (1951) has found that in the eastern Mediterranean and some other places there is a daytime maximum of thunderstorm frequency near coasts which project into the sea, and a night-time maximum near coasts which are concave towards the sea and which presumably cause a convergent land-breeze flow to develop over the coastal water. Brückner (1951) has described the regular sequence of daytime hill thunderstorms and night-time valley storms in Colombia, and Bleeker and Andre (1951) propose that the nocturnal maximum of thunderstorm occurrence over the central plains of the U.S.A. is due to the same process operating on an even larger scale.

Presumably widespread layer clouds are first formed in the lower layers and provide the heat source for the assembly of large cumulus clouds, but the details of the development of the cloud systems of these nocturnal thunderstorms have not been investigated and are not understood.

7.7 *Forecasting the development of cumulus clouds*

We see that over sea and land alike the development of cumulus clouds is powerfully controlled by large-scale flow patterns associated with a general ascent or subsidence. When the clouds are widespread the details of the process of cumulus convection, together with these large-scale movements, determine the depth of the convective layer and the distribution of temperature and humidity within it, and thereby also the distribution of buoyancy, updraught speed and condensed-water content inside the clouds. Even when the relations between the latter properties and the sounding representative of the clear-air environment have been established, it will not be profitable to attempt forecasting the one directly from the other, except perhaps in those instances over land where the convective clouds grow over isolated sources, occupy a very small fraction of the sky and do not exert much control over the properties of their surroundings. To make a successful forecast of cumulus development information will be required on the present condition of the air mass to be heated, on the rate of heating and other surface features which are likely to determine the characteristics of the cloud population (well-scattered, or locally concentrated over favourable sources), and the future large-scale vertical motion. The influence of the last feature is so strong that in the present state of knowledge consideration of the synoptic situation is more important than the detailed examination of recent soundings and temperature changes. This is recognized in forecasting practice, though not perhaps so widely in the literature. However, it has often been expressed by Douglas (e.g., 1920), and recently Byers and Rodebush (1948), in discussing the importance of sea-breeze convergence as a cause of the frequent thunderstorms in Florida, mention that they could find no clear relation between soundings and the development of thunderstorms. Means (1952) has related thunderstorm occurrence to the cross product of the pressure and temperature gradients on 850-mb charts, i.e., in effect to the baroclinity of the lower atmosphere, which he recognizes is probably more pronounced in regions where there is general vertical motion. Sutcliffe (1952) says that ' a forecaster will give a good estimate of convective activity from his prebaratic chart . . . . without even troubling his head about the probable lapse rates.' This does not of course imply that he is as successful as he would wish.

The possibility of relating the convection to the isobaric pattern suggests that numerical methods of computing the pressure changes may eventually be able to include the heating due to convection in the equations without any mathematical formulation of the process of convection.

8. Conclusion

8.1 *Requirements of improved theories of cumulus convection*

New theories of cumulus convection are required to account for the properties of individual clouds and for the general depth and structure of convective layers. In the first place some model is needed of the air motion which occurs when a bubble of cloud air ascends through a dry environment. The manner and the effects of mixing determine the height through which it rises before evaporation, and the distribution within it of updraught speed and concentration of condensed water. Next, the character of cumulus populations must be examined; the distribution and strength of the thermal sources,

together with the large-scale weather features and processes attending the growth and decay of individual clouds, determine the depth and the degree of the modification of the air mass by the convection, and also the size and vigour of the cumulus clouds.

The difficulty of the problem is as great as that in any other aspect of meteorology, but because the convective phenomena are so important and spectacular there is no scarcity of workers to tackle it, and we may expect steady progress.

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The dynamical influence of large-scale heat sources and sinks on the quasi-stationary mean motions of the atmosphere

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Summary

The classical convective theory for the influence of large-scale zonally asymmetric heating and cooling on the quasi-stationary mean motions is re-examined and shown to be inconsistent with observation. Scale considerations indicate that the classical mechanism cannot be applied to heating on a scale larger than that responsible for sea-breezes. In analogy with the theory of orographically produced stationary disturbances, heat sources and sinks of planetary dimensions produce quasi-static and quasi-geostrophic perturbations. A thermally active model is constructed in which all motions except those of large scale are filtered out *a priori*. The small perturbations produced by an idealized large-scale distribution of heat sources and sinks acting on a horizontally uniform baroclinic zonal current confined between rigid horizontal planes are then studied. The influence of surface friction is taken into account. Calculations are also performed on a model with no friction and on the assumption that entropy is advected horizontally. The results generally show good agreement with those for the more general model. When the stratosphere is taken into account, the disturbances in the upper troposphere are reduced in magnitude. The conditions for resonance in the absence of friction are determined. The passage through the resonant state is shown to modify significantly the character of the motions. The observed normals are discussed at some length in light of the theoretical computations. It is found that the magnitude and spatial distribution of the model disturbances display substantial correspondence with observation when allowance is made for orographic effects. Finally a method is suggested for quantitatively deducing the three-dimensional field of non-adiabatic heating and cooling from the observed normal maps by dynamical considerations.

1. Introduction

In an attempt to account for weather changes over periods of the order of 10 to 30 days, it seems logical to try first to understand the accompanying evolutions in the large-scale flow pattern. One of the more successful techniques for long-range forecasting has involved subjective predictions of the variations of tropospheric flow patterns averaged in time over a period of 30 days (Namias 1948). The disturbances on 30-day mean maps are observed to change in a slow and regular manner. Furthermore, it has been observed by Namias (1952) that from December to March and from June to September the departures from normal of these monthly mean maps exhibit a great deal of persistence, with rapid changes occurring just after the equinoxes. This persistence for periods of about four months in midwinter and midsummer is also noted in normal flow patterns. It therefore appears that an explanation of the normal motions would be of fundamental value in deducing the mechanism of the transient motions.

Examination of daily upper-air maps shows that those components of the long planetary waves containing most of the perturbation energy travel with a phase speed so large that one would expect the averaging process to produce a substantial reduction in their amplitudes. The fact that disturbances of considerable amplitude still appear on the averaged maps would seem to imply that the stationary components of the wave motion are excited by geographically fixed sources of disturbance. Variations from month to month in the perturbation pattern of the monthly mean maps could be attributed to changes of the position and intensity of the sources of disturbance, or to changes in the zonally averaged properties of the flow, or to a combination of both. It has been suggested

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by various investigators that orography (e.g., Rossby 1939, Haurwitz 1940, Charney and Eliassen 1949, Bolin 1950), heat sources and sinks (e.g., Rossby 1939, Haurwitz 1940, Sutcliffe 1951, Bleeker 1950, Wippermann 1951), or variable surface friction at coast lines (e.g., Eady and Sawyer 1951) are responsible for at least a portion of these disturbances.

The role of mountains in determining the normal motions has been studied quantitatively by Charney and Eliassen (1949). They considered the orographically induced steady motions along a single latitude of an equivalent barotropic atmosphere of finite lateral extent and with a constant zonal velocity. The basic current was assumed to be unaffected by friction but the perturbations were subjected to a spatially constant eddy diffusivity at the lower boundary of the atmosphere. This model, when applied to the January normal 500-mb flow at 45° latitude, displayed a surprising degree of success in predicting the magnitude and position of the observed disturbances despite the rather restrictive kinematical and dynamical constraints placed upon the motions. Charney and Eliassen discounted the influence of heat sources and sinks on the 500-mb flow on the grounds that no reversal in phase of the fixed perturbations is observed to occur at this level from winter to summer.

Since Charney and Eliassen found the effects of friction to be of considerable importance, there remained the possibility that the spatial variation of the earth's roughness might produce important modifications in the motion. The present writer (in an unpublished study) has investigated the effect of variable surface friction on the orographically produced disturbances at 500 mb and has found that it results in a change of only about 10 per cent from the case of constant friction. This is to be expected since friction only influences the motions appreciably if it is acting over a long period of the air parcel's history of travel. Charney and Eliassen found that if the constant eddy diffusivity were increased fourfold everywhere the change in the stationary flow would be considerable. However, if the eddy coefficient increases by this much or even more in passing over the west coast of a continent of 5,000 km to 10,000 km breadth, but decreases again at the east coast, then in the equilibrium state the resulting motions will not differ by much from those for which the underlying surface was uniformly rough with a coefficient of friction equal to the average value in the variable case.

There is still considerable speculation about the manner in which the energy of solar radiation is made available for driving the atmosphere. That the atmosphere is relatively transparent to the incoming short waves means that the major portion of this energy is not *directly* utilized. Hence there will be a meridional gradient of heating due not only to the variation of angle of incidence, but also to the meridional variation of the zonally-averaged properties of the atmosphere and earth's surface below. The zonal mean potential energy thus acquired is then transformed to maintain the general circulation, probably by some mechanism which depends on the hydrodynamic stability properties of the flow itself. However, it is the purpose of the present study to determine how zonal asymmetries of heating affect the quasi-stationary perturbations. The use of linear arguments will make it possible to consider this problem separately, in the first approximation, from that of the maintenance of the general circulation.

Some investigators have assumed that differential heating generates meridional cellular circulations of the Hadley type which serve to maintain the zonal kinetic energy of the general circulation. However, it has been suggested by Rossby (1949) and Starr (1951) on the basis of recent observational evidence that the maintenance of the general circulation is accomplished primarily by horizontal exchange processes. Thermal circulations in vertical planes, classically, have also been postulated as the mechanism by which zonally asymmetric heat sources and sinks in middle latitudes produce the large-scale

troughs and ridges which appear on normal maps, especially the monsoonal flow patterns. The classical theory may be traced back as far as 1686, when Halley accounted for the trade winds and monsoons by the ascent of warm air in the hotter regions of the globe. Hadley, in 1735, modified Halley's explanation by taking into account the influence of the earth's rotation. The classical reasoning parallels the theory which is used to account for land and sea breezes (see for instance Willett 1944).

In the case of the Asiatic winter monsoons, it follows from the classical argument that the surface Siberian anticyclone will occur at the place of maximum cooling. From the first law of thermodynamics it can be seen that at the surface, where the vertical velocity is zero, the heat change per unit mass in the steady state is proportional to the horizontal temperature advection. One can thus deduce the locations of those heat sources or sinks in the lowest kilometre. Inspection of the January normal sea-level map (Fig. 6 (a)) indicates that the maximum warm-air geostrophic advection (which should balance the external non-adiabatic cooling) is not found at the maximum pressure but approximately 30° to the west. This phase lag may also be observed in the case of the heating off the east coasts of Asia and North America in winter and the Aleutian and Icelandic depressions, respectively. The maximum temperature, however, is found to the east of the low centres and the minimum temperature is found to the east of the high centres. The classical mechanism therefore does not seem to explain what is observed. It is proposed to show in this study that the apparent success of the usual monsoon explanation is due to a fortunate compensation of two errors, namely, a faulty primary mechanism applied to an incorrectly placed field of heat sources and sinks.

2. The effect of the scale of heating and cooling on the dynamics of the motions

Since the atmosphere is capable of sustaining a wide spectrum of disturbance frequencies, it seems reasonable to ask whether the forces which primarily govern the propagation of the disturbances are the same for all scales. That this is not so has been realized intuitively by meteorologists for many years. Charney (1948), in a recent theoretical investigation, has not only demonstrated which forces are irrelevant as far as disturbances of planetary proportions are concerned but also offered a rationale for the *a priori* filtering of the equations of motion so that they would apply only to motions of planetary dimensions. For our present purpose we would like to know, for an external source of disturbance of certain scale, what forces will be primarily responsible for the nature of the perturbation which is created in the flow? A rather complete analysis has been made by Queney (1948) for the case of orographically produced disturbances. He has shown that in a stratified rotating atmosphere moving with a constant zonal speed U, a number of distinct types of stationary waves can be produced by a cylindrical mountain range whose length along a meridian is much greater than its half-width δ:

(i) If $\delta \simeq 1$ km to 10 km, then short stationary gravity waves are produced whose wavelength is determined by U and the static stability. The rotation of the earth is unimportant. The disturbance does not contain a meridional component of motion.

(ii) If $\delta \simeq 100$ km, a complex system of stationary gravity-inertia waves is initiated with a horizontal wavelength depending on the Coriolis parameter and U, but with a vertical wavelength determined by U and the static stability. These waves already behave quasi-statically (Queney 1947, p. 63). There is now a meridional component of motion, but it is not geostrophic. The flow is quasi-horizontal.

(iii) If $\delta \simeq 1{,}000$ km, there is a system of quasi-static and quasi-geostrophic planetary waves with length essentially determined by U and the latitudinal variation of the Coriolis parameter. The flow is quasi-horizontal.

The above results should apply equally well to other types of sources, and in particular to heat and cold sources. For instance, Malkus and Stern (1951) and Stern and Malkus (1952) have studied the motions resulting from an infinite-strip island 10 km in width and normal to the mean flow. The resulting mathematical model is essentially the same as those constructed by Queney (1947) and Scorer (1949) for the mountain-wave problem of scale comparable to that given in (i). Therefore, on the basis of Queney's classification, one can infer a corresponding classification of heat sources and sinks with respect to the types of stationary disturbances that they will generate :

(i) Heating on the scale that produces convection and small-scale sea breezes.

(ii) Continental coastal heating which induces large-scale sea breezes of the type considered by Haurwitz (1947).

(iii) Heat sources and sinks of planetary dimensions which produce large-scale disturbances such as the monsoons. These will be dealt with in the present paper.

3. The observed distribution of heat sources and sinks

The atmosphere receives or gives off heat essentially in the following ways :

(i) Release of latent heat through condensation within the atmosphere. This heat is made available by the transport of water vapour through the lower boundary.

(ii) Turbulent transport of sensible heat through the lower boundary to or from the earth's surface.

(iii) Long-wave radiation through the lower boundary to or from the earth's surface.

(iv) Short-wave radiation through the upper boundary from the sun.

(v) Long-wave radiation through the upper boundary to space.

Horizontal turbulent transport of heat *within* the atmosphere is not included since it serves only to redistribute heat energy which has already been absorbed by the atmosphere.

Since the various types of heating cannot be observed directly, they must be deduced by means of theoretical relationships from physical quantities which are directly measurable. Incomplete theory and inadequate observation have hampered efforts to determine in detail the seasonal, geographical, and vertical distributions of the sources and sinks of heat in the atmosphere. However, some progress has been made and much research at present is being directed toward this problem.

The release of latent heat of condensation in a vertical air column is perhaps the simplest of the sources to evaluate. Although heat is released during cloud formation, many clouds ultimately evaporate, thus absorbing an equivalent amount of heat from the atmosphere. Since these clouds move with the air motion there is no net heating. We assume that precipitation occurs soon after condensation. Therefore, a measure of the heat permanently given up to the atmosphere is the precipitation, P, reaching the ground. The amount of heat is $S_p = L_t P$, where L_t is the latent heat of condensation. If the precipitation reaches the ground as snow, then the latent heat of sublimation must be used instead of L_t. Jacobs (1951) has computed S_p seasonally for the oceans of the northern hemisphere. This may be done similarly for the continents from the rainfall data given by Landsberg (1945). A composite chart of the heat of condensation liberated for the winter season December to February is shown in Fig. 1 (a) and for the summer season June to August in Fig. 1 (b). Since the assumption that all precipitation is in the form of rain results in an error of only about 12 per cent when snow falls (Jacobs 1949), this difference was ignored.

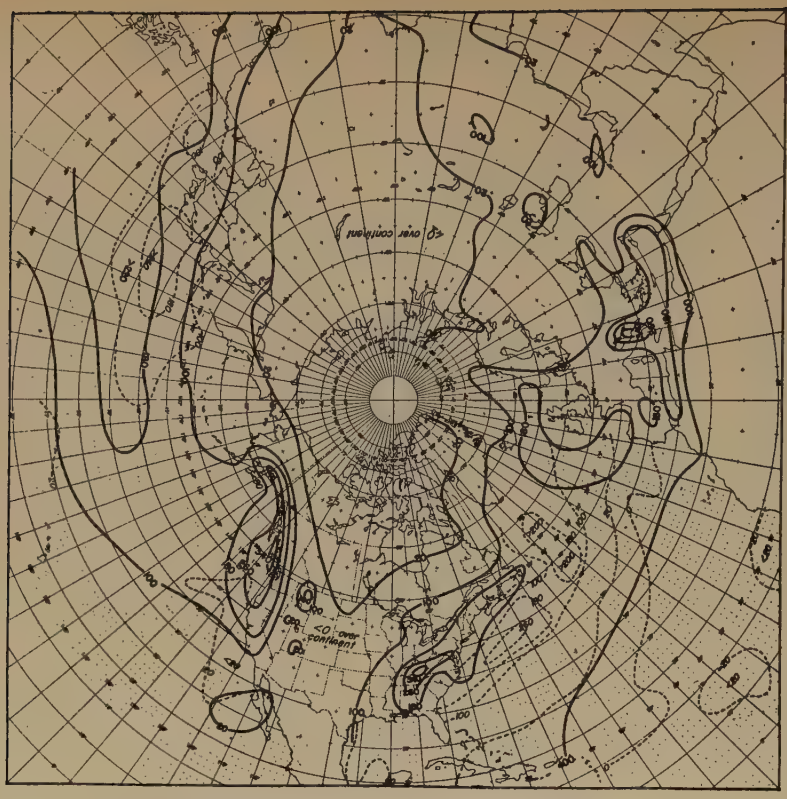

Figure 1 (a). Winter : December to February.

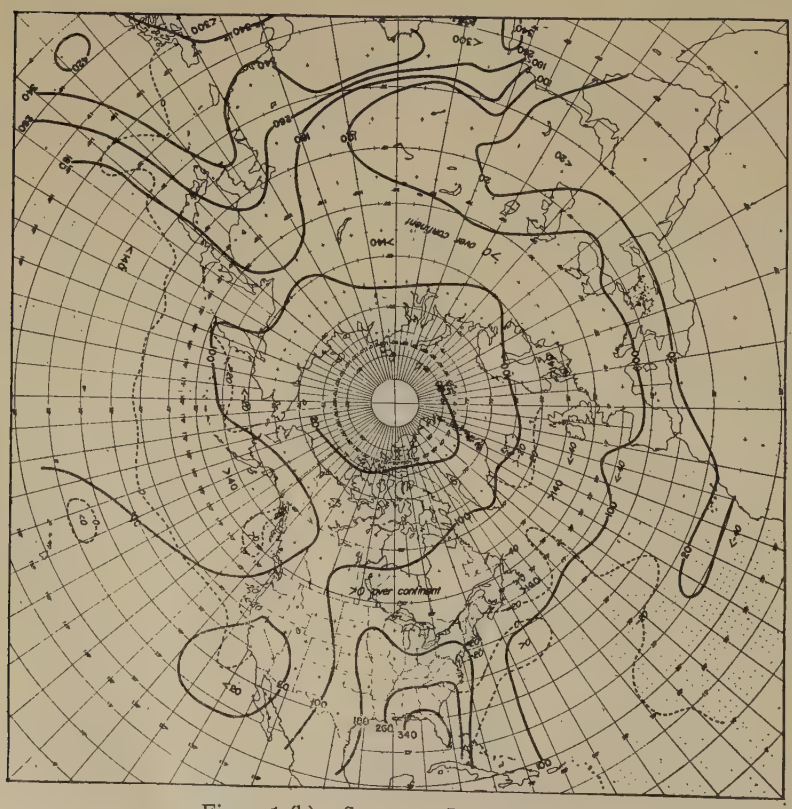

Figure 1 (b). Summer : June to August.

Figure 1. Normal seasonal non-adiabatic heating and cooling (cal cm^{-2} day^{-1}). Heat of condensation is shown by solid lines and bold type, vertical eddy conduction of sensible heat by dashed lines and italics. Oceanic data after Jacobs (1951), continental data are deduced from Landsberg (1945).

Jacobs (1943) has also calculated the sensible heat transported into the atmosphere from the sea surface; this is also shown in Figs. 1 (a) and 1 (b). Variations in heat thus made available over the oceans are primarily due to the warm and cold ocean currents. Calculations of seasonal eddy conduction over land are not available, although numerous local studies are being conducted, particularly in England and Australia. Generally, one may say that the eddy flux of heat over continental masses is downward in winter and upward in summer.

It has been shown (Elsasser 1942 and Panofsky 1947) that heat changes due to nocturnal radiation are small compared with the turbulent transfer of heat and are negligible at heights above about 50 m. The exception may occur over large continental masses which are snow-covered, for instance, Asia and Canada in the winter. Here the net radiative flux is from the air in the lowest layers to the ground (Wexler 1936) and thus is in the same direction as the eddy flux of sensible heat in winter.

The short-wave radiation from the sun is absorbed by ozone, water vapour, clouds, and by solid impurities in the atmosphere. Although the amount of this heating is appreciable, it is probable that its variation with longitude is small compared with the variations due to condensation, eddy conduction, and long-wave radiation from the earth's surface. It will be shown that in the model considered only those sources and sinks which have appreciable deviations from their zonal means give rise to disturbances in the zonal flow.

The net upwardly directed infra-red flux is considerable and is a major heat sink of the atmosphere for all seaons at all latitudes. However, this agency is also relatively uniform along latitudes.

Möller (1950a) has calculated the vertical distribution of heat added to or removed from the atmosphere in middle latitudes due to condensation (corresponding to an annual precipitation of 1,000 mm) and to radiative heat transfer under the condition of mean cloudiness. Möller's calculations show relatively little heating at the ground due to condensation; however, the amount of heating increases rapidly upward to a maximum at about 2 km and then decreases again to zero at 10 km. We also know that the magnitude of heating or cooling due to vertical eddy conduction and nocturnal radiation at any one place is a maximum at the bottom of the atmosphere and decreases rapidly with height. It appears, therefore, that the sources and sinks of heat which are mainly responsible for producing disturbances in the flow, occur at or below the condensation level, i.e., roughly 6 km.

Thus far we have attempted to describe, on the basis of existing direct or indirect observational evidence, where the various agencies inject or remove heat from the atmosphere. In motions that are averaged in time, Reynolds eddy stresses are generated which serve to redistribute momentum and heat. The magnitude of these stresses is determined by the degree of correlation of the velocity components with each other and with entropy. The modification produced by large-scale mixing on the influence of the heat sources and sinks on the normal motions has been considered by Elliott and Smith (1949) and Möller (1950b). In the present study only the direct three-dimensional heating effects, in idealized form, are calculated. The variations with longitude of the large-scale eddy stresses will not be considered.

Of course, the heat sources and sinks described above are determined to some extent by the disturbed motion itself. Certainly the distribution of water vapour, cloudiness, precipitation, static stability, and the field of motion interact to produce the average conditions. Since we are dealing with stationary motions it is permissible to specify the field of heating and cooling, and the problem will be to find the field of motion which is dynamically consistent with it.

4. A THEORETICAL HEATING MODEL

A stationary zonally-symmetric flow subjected to steady zonally-symmetric heating and cooling will eventually reach an equilibrium state that is also zonally-symmetric. On the other hand, if the sources and sinks of heat vary longitudinally, then we must expect disturbances on the zonal current. The observational evidence in Section 3 indicates that some of the sources and sinks of heat have appreciable zonal asymmetry, especially those that exist in the lowest 6 km of the atmosphere. We shall idealize the horizontal distribution of perturbation heating by assuming a periodic variation in the latitudinal and longitudinal directions.

With the assumption that $w\,\partial u/\partial z$ and $w\,\partial v/\partial z$ in the horizontal equations of motion are small compared with the horizontal advection (Charney 1948), the vorticity equation reads

$$\frac{d_h(\zeta+f)}{dt}+(\zeta+f)\,\nabla_h\cdot\mathbf{v}_h=-\mathbf{k}\cdot\nabla_h\left(\frac{1}{\rho}\right)\times\nabla_h p, \quad . \quad . \quad (1)$$

where d_h/dt is the individual horizontal time-rate of change following a material particle, ∇_h is the horizontal gradient, $\mathbf{v}_h$ is the horizontal velocity vector, ζ is the vertical component of relative vorticity, f is the Coriolis parameter, $\mathbf{k}$ is the vertical unit vector, ρ is the density, p is the pressure. With use of the geostrophic approximation, the right side, which represents the pressure volume solenoids with vertical axes, becomes $-f\,\mathbf{v}_h\cdot\nabla_h\ln\rho$. We assume small perturbations on a zonal current U which is constant with latitude and varies linearly with height, i.e., $U(z)=U(0)+\Lambda z$. Upon applying the equation of continuity and requiring steady motions, we have from Eq. (1):

$$U\frac{\partial\zeta}{\partial x}+\beta v=\frac{f}{\rho}\frac{\partial(\rho w)}{\partial z}.$$

Here v, w, and ζ are perturbation quantities, ρ and U are zonally averaged quantities, and $\beta=df/dy$. x is positive eastward, y is positive northward, z is positive upward and u, v, and w are the respective velocity components. Hereafter the variation of f is insignificant and f will therefore be assumed constant. Upon evaluating $\partial\zeta/\partial x$ geostrophically we have

$$U\nabla_h^2 v+\beta v=\frac{f}{\rho}\frac{\partial(\rho w)}{\partial z} \quad . \quad . \quad . \quad (2)$$

We can obtain an additional differential relation between v and w which involves the non-adiabatic heating. From the first law of thermodynamics and the definition of potential temperature θ we have

$$\left(\frac{\partial}{\partial t}+\mathbf{v}_h\cdot\nabla_h+w\frac{\partial}{\partial z}\right)\ln\theta=\frac{1}{c_p T}\frac{dq}{dt}\equiv Q \quad . \quad . \quad (3)$$

in which T is the temperature, dq/dt is the rate of change of heat per unit mass, c_p is the specific heat of air at constant pressure and thus $c_p Q$ is the rate of change of entropy per unit mass. Eq. (3) will be referred to as the *thermodynamic energy equation*. The thermal wind may be approximated in terms of θ:

$$\frac{\partial\mathbf{v}_h}{\partial z}\simeq\frac{g}{f}\mathbf{k}\times\nabla_h\ln\theta \quad . \quad . \quad . \quad . \quad (4)$$

For steady motions (3) and (4) give

$$\left(u\frac{\partial v}{\partial z}-v\frac{\partial u}{\partial z}\right)+\frac{g}{f}\frac{\partial\ln\theta}{\partial z}w=\frac{gQ}{f} \quad . \quad . \quad . \quad (5)$$

Again assuming small perturbations, Eq. (5) becomes

$$U\frac{\partial v}{\partial z} - \Lambda v + \frac{g}{f}\frac{\partial \ln \theta}{\partial z} w = \frac{gQ}{f} \quad . \quad . \quad . \quad (6)$$

Here Q is a perturbation quantity, and $\partial \ln \theta/\partial z$ is a mean quantity.

We assume a constant lapse rate γ,

$$T(y, z) = T(y, 0) - \gamma z \quad . \quad . \quad . \quad . \quad (7)$$

However, the vertical variation of the zonally-averaged pressure is given to sufficient accuracy in terms of the mean temperature $\overline{T}$ by

$$p(y, z) \simeq p(y, 0) \exp(-z/H), H \equiv R\overline{T}/g \quad . \quad . \quad . \quad (8)$$

The vertical variation of the zonally-averaged density may be determined by differentiating the equation of state with respect to z, then using (7) and (8) and integrating again with the assumption that $\overline{T}$ may replace T when it appears as a coefficient.

$$\rho(y, z) \simeq \rho(y, 0) \exp(-z/H^*), H^* \equiv \overline{T}\Big/\left(\frac{g}{R} - \gamma\right) \quad . \quad . \quad (9)$$

where R is the gas constant for air. Hence the frequency of a buoyancy oscillation ν is given by,

$$\nu^2 \equiv g\frac{\partial \ln \theta}{\partial z} \simeq \frac{g}{\overline{T}}(\gamma_d - \gamma), \quad . \quad . \quad . \quad . \quad (10)$$

where γ_d is the adiabatic lapse rate.

We now assume the perturbation heat sources and sinks to vary sinusoidally with x and y :

$$Q(x, y, z) = \chi(z) \sin kx \sin \mu y \quad . \quad . \quad . \quad (11)$$

where $k = 2\pi/L$ and $\mu = 2\pi/D$, L and D being respectively the longitudinal and the lateral wavelengths. Consistent with this variation, v and w must have the form

$$\left.\begin{aligned} v(x, y, z) &= [V_1(z) \sin kx + V_2(z) \cos kx] \sin \mu y \\ w(x, y, z) &= [W_1(z) \sin kx + W_2(z) \cos kx] \sin \mu y \end{aligned}\right\} \quad . \quad (12)$$

in which $V_1(z)$, $V_2(z)$, $W_1(z)$, and $W_2(z)$ are to be determined. Upon substituting (11) and (12) into (2) and (6) and equating coefficients of $\sin kx$ and $\cos kx$, there results a system of two pairs of ordinary differential equations which can be solved simultaneously to eliminate $W_1(z)$ and $W_2(z)$. We obtain

$$\left.\begin{aligned} \frac{d^2 V_1}{dz^2} - \frac{1}{H^*}\frac{dV_1}{dz} - a^2\left(\frac{U - U_c^*}{U}\right) V_1 &= \frac{g}{f\rho U}\frac{d(\rho\chi)}{dz} \\ \frac{d^2 V_2}{dz^2} - \frac{1}{H^*}\frac{dV_2}{dz} - a^2\left(\frac{U - U_c^*}{U}\right) V_2 &= 0 \end{aligned}\right\} \quad . \quad . \quad (13)$$

where we have abbreviated :

$$\left.\begin{aligned} U_c &= \beta/(\mu^2 + k^2) \\ a^2 &= (\mu^2 + k^2)\nu^2/f^2 \\ U_c^* &= U_c + \Lambda/(a^2 H^*) \end{aligned}\right\} \quad . \quad . \quad . \quad (14)$$

We shall consider the effect of surface friction, but only as it acts on the perturbations. The energy dissipated in this manner is a second-order quantity. Since we have taken

a periodic distribution of relative heat sources and sinks, there is no net heat-energy contribution. Therefore, the frictional loss of energy must be balanced by appropriate second-order changes in the energy of the basic current. Thus there is no ambiguity in assuming that the zonal velocity is steady to the first order of small quantities.

The introduction of surface friction into the baroclinic model may be accomplished by assuming the friction layer to be a barotropic boundary layer. The justification of this lies in the fact that the variation of the wind with height due to baroclinicity in the friction layer is probably small compared with that due to the surface stress. As has been demonstrated by Charney and Eliassen (1949), vertical velocities are generated at the top of a barotropic friction layer which are proportional to the perturbation vorticity at this level. If we now let $z = 0$ be this height, then the above statement constitutes a lower boundary condition for our model :

$$w(x, y, 0) = HF\zeta(x, y, 0)/f \quad . \quad . \quad . \quad . \quad (15)$$

in which

$$F = \frac{\sin 2\alpha}{H}\left(\frac{Kf}{2}\right)^{\frac{1}{2}} \quad . \quad . \quad . \quad . \quad . \quad (16)$$

α is the angle between the isobars and the surface wind, and K is the eddy diffusivity. F will be taken as constant and we will adopt the mean values suggested by Brunt (1939) : $\alpha = 22{\cdot}5°$ and $K = 10\ \mathrm{m^2\ sec^{-1}}$, which with $f = 10^{-4}\ \mathrm{sec^{-1}}$ and the height of the homogeneous atmosphere $H = 8\ \mathrm{km}$ yields $F = 2{\cdot}0 \times 10^{-6}\ \mathrm{sec^{-1}}$.

For the upper boundary condition it would be desirable to simulate the stabilizing influence of the stratosphere on the troposphere without taking the stratosphere into account explicitly. A rigid top, which is equivalent to a stratosphere of infinite static stability, would result in large mass divergences just below the tropopause. This is especially true if an extremum in vertical velocity occurs near the upper boundary. We shall first assume a rigid top : $w = 0$ at $z = z_T$. We therefore must expect that the amplitudes of the disturbance in the upper portion of the model will be somewhat larger than if we had assumed a moderately stable fluid layer in place of the rigid top.

Using (11) and (12) in the energy equation (6) and imposing the boundary conditions (15) and $w = 0$ at $z = z_T$, we have at $z = 0$:

$$\left.\begin{aligned} U\frac{dV_1}{dz} - \Lambda V_1 - \frac{\nu^2 HF(\mu^2 + k^2)}{f^2 k} V_2 &= \frac{g\chi}{f} \\ U\frac{dV_2}{dz} - \Lambda V_2 + \frac{\nu^2 HF(\mu^2 + k^2)}{f^2 k} V_1 &= 0 \end{aligned}\right\} \quad . \quad . \quad (17)$$

and at $z = z_T$:

$$\left.\begin{aligned} U\frac{dV_1}{dz} - \Lambda V_1 &= \frac{g\chi}{f} \\ U\frac{dV_2}{dz} - \Lambda V_2 &= 0 \end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (18)$$

Let $a^{*2} = a^2 + (2H^*)^{-2}$, then system (13), (17) and (18) may be transformed by

$$\left.\begin{aligned} \phi_1(z) &= V_1(z) \exp\left[(a^* - (2H^*)^{-1})\, z\right] \\ \phi_2(z) &= V_2(z) \exp\left[(a^* - (2H^*)^{-1})\, z\right] \\ \xi(z) &= 2a^*\left(z + \Lambda^{-1} U(0)\right) \end{aligned}\right\} \quad . \quad . \quad . \quad (19)$$

to give

$$\left.\begin{aligned} \xi\phi_1'' - \xi\phi_1' + r\phi_1 &= \Pi(\xi) \\ \xi\phi_2'' - \xi\phi_2' + r\phi_2 &= 0 \end{aligned}\right\}, \quad \xi_0 \leqslant \xi \leqslant \xi_T \qquad (20)$$

$$\left.\begin{aligned} \phi_1' - \lambda\phi_1 - \sigma\phi_2 &= \kappa \\ \phi_2' - \lambda\phi_2 + \sigma\phi_1 &= 0 \end{aligned}\right\}, \quad \xi = \xi_0 \qquad (21)$$

$$\left.\begin{aligned} \phi_1' - \lambda\phi_1 &= \kappa \\ \phi_2' - \lambda\phi_2 &= 0 \end{aligned}\right\}, \quad \xi = \xi_T \qquad (22)$$

where a prime indicates ordinary differentiation with respect to ξ and we have abbreviated:

$$\left.\begin{aligned} r &= a^2 U_c^*/(2\, a^*\, \Lambda) \\ \Pi(\xi) &= \frac{g(\rho\chi)'}{f\Lambda\rho} \exp\left[\left(\tfrac{1}{2} - \frac{1}{4a^* H^*}\right)\left(\xi - \xi_0\right)\right] \\ \lambda(\xi) &= \tfrac{1}{2} - \frac{1}{4a^* H^*} + \frac{1}{\xi} \\ \kappa(\xi) &= g\chi(\xi)/(f\Lambda\xi) \\ \sigma &= \nu^2 HF(\mu^2 + k^2)/(kf^2\Lambda\xi_0) \end{aligned}\right\} \qquad (23)$$

The homogeneous portion of (20) is of the same form as the differential equation derived by Charney (1947) in connection with a study of baroclinic instability. In this paper he gave a thorough discussion of the solutions to this form of the confluent hypergeometric differential equation. With Charney's notation $\psi_2(\xi, r)$ is the solution of the *first* kind and $\psi_1(\xi, r)$ is a linearly independent solution of the *second* kind.

The general solutions of (20) are

$$\left.\begin{aligned} \phi_1(\xi) &= A(\xi)\,\psi_1(\xi) + B(\xi)\,\psi_2(\xi) \\ \phi_2(\xi) &= E_1\,\psi_1(\xi) + E_2\,\psi_2(\xi) \end{aligned}\right\} \qquad (24)$$

in which

$$\left.\begin{aligned} A(\xi) &= \int_{\xi_0}^{\xi} \frac{\Pi(\xi)\,d\xi}{\xi\,\psi_2\,(\psi_1/\psi_2)'} + A(\xi_0) \\ B(\xi) &= -\int_{\xi_0}^{\xi} \frac{\psi_1\,\Pi(\xi)\,d\xi}{\xi\,\psi_2^2\,(\psi_1/\psi_2)'} + B(\xi_0) \end{aligned}\right\} \qquad (25)$$

$A(\xi_0)$, $B(\xi_0)$, E_1, and E_2 are fixed by the boundary conditions and may be determined by applying (24) and (25) to (21) and (22):

$$\left.\begin{aligned} E_1 &= \frac{C_4\left[(C_4 C_7 - C_3 C_8)(C_3\,\kappa(\xi_0) + C_1 G) + (C_2 C_3 - C_1 C_4) C_7 G\right]}{C_3\left[(C_2 C_3 - C_1 C_4)^2 + (C_4 C_7 - C_3 C_8)^2\right]} \\ E_2 &= -\frac{C_3 E_1}{C_4} \\ B(\xi_0) &= \frac{C_3(C_1 E_1 + C_2 E_2) + C_7 G}{C_4 C_7 - C_3 C_8} \\ A(\xi_0) &= \frac{G - C_4 B(\xi_0)}{C_3} \end{aligned}\right\} \qquad (26)$$

where

$$\left.\begin{aligned} C_1 &= \psi_1'(\xi_0) - \lambda(\xi_0)\,\psi_1(\xi_0) \\ C_2 &= \psi_2'(\xi_0) - \lambda(\xi_0)\,\psi_2(\xi_0) \\ C_3 &= \psi_1'(\xi_T) - \lambda(\xi_T)\,\psi_1(\xi_T) \\ C_4 &= \psi_2'(\xi_T) - \lambda(\xi_T)\,\psi_2(\xi_T) \\ C_5 &= A(\xi_T) - A(\xi_0) \\ C_6 &= -[B(\xi_T) - B(\xi_0)] \\ C_7 &= \sigma\,\psi_1(\xi_0) \\ C_8 &= \sigma\,\psi_2(\xi_0) \\ G &= \kappa(\xi_T) - C_3\,C_5 + C_4\,C_6 \end{aligned}\right\} \quad . \quad . \quad . \quad (27)$$

In order to simulate the observed vertical variation of heating we choose an analytic expression for χ satisfying the requirement that it have a maximum in mid-troposphere and vanish at the tropopause :

$$\chi = N\,e^{-z/h}\sin(\pi z/z_T) \quad . \quad . \quad . \quad . \quad (28)$$

In particular, this form of χ vanishes at $z = 0$ and z_T, and thus $\kappa(\xi_0) = \kappa(\xi_T) = 0$. The altitude of maximum χ is controlled by h. Our knowledge of the details of χ is rather scanty; however, we do have a good estimate of the perturbation total heat change S in a column of unit area. This quantity is related to Q by :

$$S = \int_0^{z_T} \rho \frac{dq}{dt}\,dz = \int_0^{z_T} c_p\,T\rho Q\,dz \simeq c_p\,\overline{T}\int_0^{z_T}\rho Q\,dz \quad . \quad . \quad (29)$$

The maximum value of S in the horizontal, using (11), is thus

$$S_{\max} = c_p\,\overline{T}\int_0^{z_T}\rho\chi\,dz \quad . \quad . \quad . \quad . \quad (30)$$

If we wish to vary h in (28) but keep $S_{\max}$ invariant, N must be determined by

$$N = \frac{S_{\max}}{c_p\,\overline{T}\,\rho(0)}\,\frac{\left(\frac{1}{h}+\frac{1}{H^*}\right)^2+\left(\frac{\pi}{z_T}\right)^2}{\left(\frac{\pi}{z_T}\right)\left\{1+\exp\left[-\left(\frac{1}{h}+\frac{1}{H^*}\right)z_T\right]\right\}} \quad . \quad . \quad (31)$$

Figs. 1 (a) and 1 (b) indicate that the vertical integral of the heat change is of the order of magnitude of 100 cal cm^{-2} day^{-1}. The elements of greatest uncertainty are the magnitude of the sensible heat transfer over the continents and the magnitude of the nocturnal radiation in winter over Asia and Canada. What little is known about these factors (see, for instance, London 1952) indicates that they are at most of the same order of magnitude as the contributions from heat of condensation. The value of $S_{\max}$ chosen was 0·3 cal cm^{-2} min^{-1} = 432 cal cm^{-2} day^{-1}. This probably overestimates the maximum deviation from the zonal average heating, especially in summer. However, since the magnitude of the perturbation is directly proportional to $S_{\max}$, one can easily infer the disturbance amplitude for other values of $S_{\max}$.

For the characteristic horizontal dimensions of relative heating and cooling one must look again to observations. The separation of primary maxima along a given latitude is approximately the distance between the east coasts of Asia and North America, i.e.,

160° longitude. A noteworthy exception is the wintertime-condensation source at the west coast of North America (Fig. 1 (a)). The effect of this source is probably compensated for by the upward motion along the western slopes of the Rockies. Evidence of this lies in the fact that the sea-level normal (see Fig. 6 (a) below) indicates warm-air advection along the west coast.

The wavelength of the principal Fourier component of the heat source-sink distribution in the north-south direction varies from 30° to 60° latitude. Since we are interested only in a general quantitative indication of the nature and magnitude of the disturbances it will suffice for our purposes to consider two extreme values. For computational convenience, these lengths are chosen to be 35° and 53·9° latitude. The latter value is probably closer to the average lateral width.

In order to utilize the values of the confluent hypergeometric functions which are already tabulated, the physical parameters which determine r in (26) were adjusted slightly. For greater accuracy, it was necessary to recalculate Charney's (1947) values of the derivatives.

The quantities which characterize the zonally-averaged stratified current were taken to be : $\bar{T} = 260°\text{K}$, $\gamma = 6{\cdot}5°\text{C km}^{-1}$, $\gamma_d = 9{\cdot}8°\text{C km}^{-1}$, $\Lambda = 2{\cdot}25\ \text{m sec}^{-1}\ \text{km}^{-1}$, and $U(0) = 2{\cdot}29\ \text{m sec}^{-1}$. The rigid top was placed at $z_T = 10$ km. We choose $f = 10^{-4}\ \text{sec}^{-1}$ and $\beta = 1{\cdot}61 \times 10^{-11}\ \text{m}^{-1}\ \text{sec}^{-1}$, corresponding to approximately 45° latitude. The two properties of the heating which were kept constant throughout were : $L = 160°$ longitude $= 12{\cdot}61 \times 10^3$ km at 45° latitude and $S_{\max} = 0{\cdot}3\ \text{cal cm}^{-2}\ \text{min}^{-1} = 432\ \text{cal cm}^{-2}\ \text{day}^{-1}$.

When $D = 35°$ latitude ($= 3{\cdot}89 \times 10^3$ km), $r = 0{\cdot}500$; when $D = 53{\cdot}9°$ latitude ($= 5{\cdot}99 \times 10^3$ km), $r = 0{\cdot}700$. Hereafter the following notation will be used : $D_1 = D = 35°$ latitude and $D_2 = D = 53{\cdot}9°$ latitude. Calculations were performed for D_1 with $h = 1$, 2, and 5 km, and for D_2 with $h = 2$ km. Two cases were treated : $F = 0$ and $F = 2{\cdot}0 \times 10^{-6}\ \text{sec}^{-1}$.

The results with no friction are given in Figs. 2 (a) and 2 (b). In this case $\sigma \to 0$ in (26) and in the limit $E_1 = E_2 = V_2(z) = W_2(z) \equiv 0$, so that the disturbance is in phase or exactly out of phase with respect to height. Figs. 2 (a) and 2 (b), therefore, only show $V_1 \equiv V$ and $W_1 \equiv W$.

In each case V is negative at least below 6 km, thereby implying that in this portion of the troposphere there is a trough one-quarter of a wavelength to the east of a maximum in Q and a ridge the same distance downstream from a minimum in Q. Where the heating is concentrated at lower levels (smaller h), the maximum in V is larger and occurs

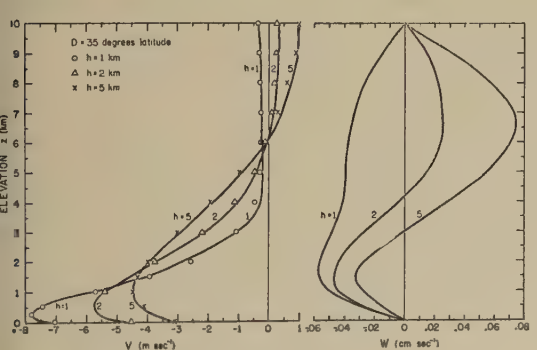

Figure 2 (a). $D = 35°$ latitude ($\equiv D_1$).

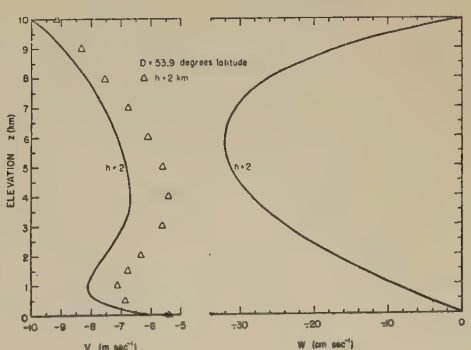

Figure 2 (b). $D = 53{\cdot}9°$ latitude ($\equiv D_2$).

Figure 2. Vertical distribution of perturbation meridional component of velocity v, and vertical component w at maximum of a heat source for different vertical distributions of heating as given by h. Tropopause taken to be rigid. Friction is not included. $L = 160°$ longitude. Solid lines are computed from general baroclinic model. Circles, triangles and crosses indicate points computed from advective model.

at a lower elevation. For the narrower lateral width, D_1, V becomes small above about 4 km for all values of h. However, for D_2, there is a minimum at this elevation, and then the disturbance increases up to the tropopause. W may be computed from either the thermodynamic energy equation or the vorticity equation. For D_1, the vertical motions are of an order of magnitude less than those usually associated with large-scale daily disturbances, but for D_2, W is of the same order of magnitude as is found in daily motions.

In the lower levels, the vertical motion is *downward* over the heat source. This can be inferred immediately by integrating the vorticity equation (2) from the ground to a level z and making use of (12) and the fact that $V_2(z) = W_2(z) \equiv 0$:

$$W = -\frac{\mu^2 + k^2}{\rho f} \int_0^z \rho V (U - U_c)\, dz \quad . \quad . \quad . \quad (32)$$

Since $U < U_c$ and $V < 0$ in the lower levels, then $W < 0$. The small vertical motions for D_1 must mean that the non-adiabatic heating is almost entirely balanced by the horizontal advection of entropy. This does not appear to be so for D_2.

The results for the model with friction are given in Figs. 3 (a), (b), and (c). (D_1 : $h = 5$ km. has been omitted). In the case of no friction, a change of phase with height of $\frac{1}{2} L$ occurred discontinuously for D_1 : $h = 2$, and not at all for D_1 : $h = 1$ km and for D_2 : $h = 2$ km. With friction the tilt with height is continuous, being rapid in the lower atmosphere and becoming almost zero in the upper levels. Here the phase shift for all the h with $D = D_1$ is approximately $\frac{1}{4} L$ to the west from the ground to 10 km. The phase shift is still less for D_2. The absolute maximum of V in the low levels is damped by friction. As in the case without friction, when the heating is concentrated near the ground a greater disturbance is produced at lower levels, but in contradistinction to the non-frictional case a greater disturbance is also produced in upper levels. Since the magnitude of the surface disturbance determines the frictionally-produced vertical velocity through the top of the friction layer, it also determines the compensating convergence or divergence in the upper atmosphere. Thus, in the case of friction the increased divergences result in increased disturbance amplitudes in the upper levels.

The vertical velocities for D_1 are an order of magnitude larger with than without surface friction, and thereby completely obscure the non-frictional effect. For the case of this narrow lateral width the maximum of downward motion is over the surface ridge and that of upward motion is over the trough. Since the non-frictional vertical velocity for D_2 was large to begin with, the introduction of friction increases the pre-existing maximum of downward motion, which now occurs at a level $1\frac{1}{2}$ km higher than for D_1, and shifts it to a position about $\frac{1}{8} L$ to the east of the surface ridge; the maximum of upward motion is similarly located with respect to the surface trough.

The phase lag between surface trough and maximum Q is now in each case less than a quarter of a wavelength. Since $L = 160°$ longitude, the trough is about 25° longitude to the east of maximum Q. When the heating is concentrated closer to the ground, the rapid change of phase with height occurs at lower levels. For D_1, the 500-mb trough lags to the west of the surface trough by a distance of about 40° longitude and is about 15° to the west of the maximum Q. The lag between surface and 500-mb trough for D_2 is somewhat less, about 25°, so that the 500-mb trough is almost directly over the maximum Q.

From Fig. 3 we see that the perturbation-pressure amplitude at the surface is of the order of 10 mb. The disturbed height of the 500-mb surface for D_1 is approximately 200 ft, but is almost four times as large for D_2. The pressure amplitudes, as well as the velocity components, are of the order of magnitude of the observed disturbances.

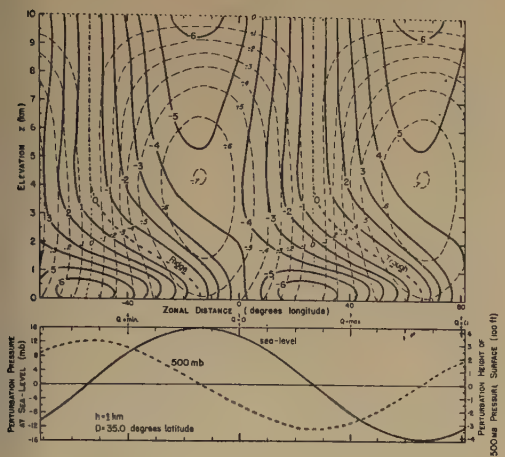

Figure 3 (a)

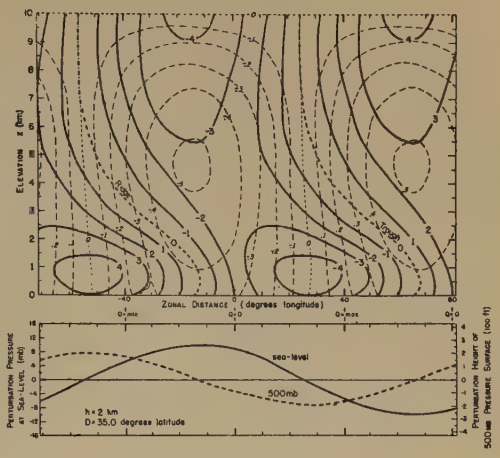

Figure 3 (b)

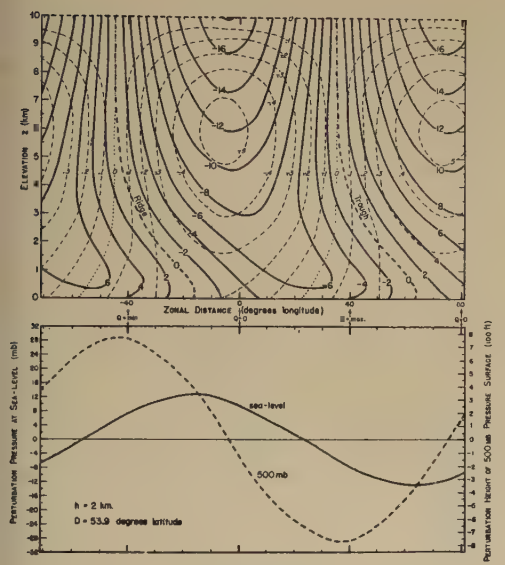
Elevation z (km)
Zonal Distance (degrees longitude)
sea-level
500 mb
h = 2 km.
D = 53.9 degrees latitude
Perturbation Pressure at Sea-Level (mb)
Perturbation Height of 500 mb Pressure Surface (100 ft)

Figure 3 (c)

Figure 3. General baroclinic model with surface friction, at latitude of maximum heating and cooling. Tropopause taken to be rigid. Upper part : zonal section of perturbation meridional velocity v (solid lines) in m sec^{-1} and perturbation vertical velocity w (dashed lines) in cm sec^{-1}. Lower part : zonal profile of perturbation pressure at sea level (solid lines) and perturbation height of 500-mb pressure surface (dashed lines).

5. The advective approximation

It is interesting to see to what extent a simpler baroclinic model is capable of determining the disturbed motions. We make the *a priori* assumption that in the steady state (3) may be written :

$$\mathbf{v}_h \cdot \nabla_h \ln \theta \simeq Q \qquad . \qquad . \qquad . \qquad . \qquad (33)$$

which is tantamount to assuming that the static stability has little influence on the motions. This advective approximation has been used by Sutcliffe (1947) in the investigation of daily motions. With this approximation, the use of the confluent hypergeometric function can be circumvented. The results of the general baroclinic calculations show that the above approximation is not valid in the case of friction since the vertical velocities are large. The advective model will therefore not be applied to the cases with friction in the following. In each case we will test the validity of (33) *a posteriori*.

With the above approximation and with $V_2 = 0$ and $V_1 \equiv V$ in (12) we have

$$\frac{1}{U^2}\left(U\frac{dV}{dz} - \Lambda V\right) = \frac{d}{dz}\left(\frac{V}{U}\right) = \frac{g\chi}{fU^2} \quad . \quad . \quad . \quad (34)$$

The vorticity equation, namely the unintegrated form of (32), is

$$\frac{d(\rho W)}{dz} = -\left(\frac{\mu^2 + k^2}{f}\right)(U - U_c)\rho V \quad . \quad . \quad . \quad (35)$$

We now follow the procedure outlined by Charney, Fjörtoft and von Neumann (1950). (34) may be integrated from $z = 0$ to z:

$$\frac{V(z)}{U(z)} - \frac{V(0)}{U(0)} = \frac{g}{f}\int_0^z \chi U^{-2}\, dz \equiv -\Upsilon(z) \quad . \quad . \quad . \quad (36)$$

We define the vertical pressure mean of a quantity:

$$\overline{\Xi} \equiv \frac{1}{p(z_T) - p(0)}\int_{p(0)}^{p(z_T)} \Xi\, dp = \frac{g}{p(0) - p(z_T)}\int_0^{z_T} \Xi\rho\, dz \quad . \quad (37)$$

The mean of the vorticity equation (35) subject to the boundary conditions $\rho W = 0$ at $z = 0, z_T$ is

$$\overline{UV} = U_c\,\overline{V} \quad . \quad . \quad . \quad . \quad . \quad (38)$$

But from (36) we have

$$\overline{UV} = \frac{V(0)}{U(0)}\,\overline{U^2} - \overline{U^2\Upsilon} \quad . \quad . \quad . \quad . \quad (39)$$

and also

$$\frac{V(0)}{U(0)} = \frac{\overline{V}}{\overline{U}} + \frac{\overline{U\Upsilon}}{\overline{U}} \quad . \quad . \quad . \quad . \quad . \quad (40)$$

We may now eliminate $\overline{UV}$ and $V(0)/U(0)$ between (38), (39), and (40), thereby obtaining an expression for V in terms of the given quantities U, Υ, and U_c:

$$\overline{V} = \frac{\overline{U^2\Upsilon} - \overline{U^2}\,\overline{U}^{-1}\,\overline{U\Upsilon}}{\overline{U^2}\,\overline{U}^{-1} - U_c} \quad . \quad . \quad . \quad . \quad (41)$$

At any level V can then be found by eliminating $V(0)/U(0)$ between (36) and (40):

$$V = U\overline{U}^{-1}(\overline{V} + \overline{U\Upsilon} - \overline{U}\Upsilon) \quad . \quad . \quad . \quad (42)$$

V was computed by performing the indicated quadratures numerically using the same mean properties of the flow as before. The results are indicated in Fig. 2 (a) for $h = 1$, 2, and 5 km and in Fig. 2 (b) for $h = 2$ km. For D_1, where the vertical velocities are small, the deviations from the results of the general baroclinic model are within the error of numerical integration. On the other hand, the larger vertical motions for $D = D_2$ produce appreciable deviations in the magnitude of V, although the vertical variation is quite similar.

From (41) we see that the perturbations can become infinite when $U_c = \overline{U^2}/\overline{U}$, that is, when the wavelength of the heating distribution is equal to the stationary wavelength of the baroclinic flow. Resonance can also occur in the general baroclinic model without friction since it too is a linear system; however, this phenomenon is avoided by introducing the dissipative effect of surface friction into the model.

6. The influence of the stratosphere

We shall now proceed to determine the tropospheric motions under the influence of an upper layer of moderate static stability. Assume the tropopause to be a material surface. Let us require that the pressure be continuous everywhere. In particular consider the pressure on either side of the tropopause. Since this is a stream surface by definition, then by combining the dynamic and kinematic boundary conditions we have

$$\frac{d^{\mathrm{I}}(p^{\mathrm{I}} - p^{\mathrm{II}})}{dt} = \frac{d^{\mathrm{II}}(p^{\mathrm{I}} - p^{\mathrm{II}})}{dt} = 0 \quad . \quad . \quad . \quad (43)$$

The superscripts I and II refer to the tropospheric and stratospheric sides, respectively. The pressure on either side of the tropopause is written as a mean and a small perturbation, $P^{\mathrm{I}} + p^{\mathrm{I}}$ or $P^{\mathrm{II}} + p^{\mathrm{II}}$. The boundary conditions (43) for stationary perturbations on a zonal current then become :

$$U^{\mathrm{I,\,II}} \frac{\partial (p^{\mathrm{I}} - p^{\mathrm{II}})}{\partial x} + v^{\mathrm{I,\,II}} \frac{\partial (P^{\mathrm{I}} - P^{\mathrm{II}})}{\partial y} + w^{\mathrm{I,\,II}} \frac{\partial (P^{\mathrm{I}} - P^{\mathrm{II}})}{\partial z} = 0 \quad . \quad (44)$$

Here the quantities are measured at the mean tropopause, but this approximation results in an error of only second order. We impose the quasi-static and quasi-geostrophic approximations on the mean and perturbed motions :

$$fU^{\mathrm{I,\,II}}(\rho^{\mathrm{I}} v^{\mathrm{I}} - \rho^{\mathrm{II}} v^{\mathrm{II}}) - fv^{\mathrm{I,\,II}}(\rho^{\mathrm{I}} U^{\mathrm{I}} - \rho^{\mathrm{II}} U^{\mathrm{II}}) - gw^{\mathrm{I,\,II}}(\rho^{\mathrm{I}} - \rho^{\mathrm{II}}) = 0 \quad . \quad (45)$$

where ρ is a mean quantity. Upon requiring that the mean zonal velocity and the mean density be continuous through the material surface, we have that

$$v^{\mathrm{I}} = v^{\mathrm{II}} \quad . \quad . \quad . \quad . \quad . \quad (46)$$

Therefore the perturbation meridional component is continuous, although $\partial U/\partial z$ or $\partial \rho/\partial z$ may undergo a jump through the surface. Let us define the tropopause as the surface

$$F(x, y, z) = z - Z(y) - \eta(x, y) = 0, \ \eta^2 \ll Z^2 \quad . \quad . \quad (47)$$

By the kinematic boundary condition, the normal velocity on either side must vanish. Thus for steady motions

$$\mathbf{v}^{\mathrm{I}} \cdot \nabla F = \mathbf{v}^{\mathrm{II}} \cdot \nabla F \quad . \quad . \quad . \quad . \quad (48)$$

so that

$$U^{\mathrm{I,\,II}} \frac{\partial \eta}{\partial x} + v^{\mathrm{I,\,II}} \frac{dZ}{dy} - w^{\mathrm{I,\,II}} = 0 \quad . \quad . \quad . \quad (49)$$

Again with $U^{\mathrm{I}} = U^{\mathrm{II}}$ we have

$$w^{\mathrm{I}} - w^{\mathrm{II}} = (v^{\mathrm{I}} - v^{\mathrm{II}}) \frac{dZ}{dy} \quad . \quad . \quad . \quad . \quad (50)$$

which relates the jump in w to that of v. But since we have shown v to be continuous then w also must be continuous :

$$w^{\mathrm{I}} = w^{\mathrm{II}} \quad . \quad . \quad . \quad . \quad . \quad (51)$$

Assume that the zonal mean temperature and velocity are constant with height in the stratosphere and are continuous at the tropopause. One can then show from the thermal-wind relation (cf., Charney 1947) that the mean tropopause must be isothermal and have a slope given by

$$\frac{dZ}{dy} \simeq \frac{f\Lambda}{\gamma g} T(z_T) \quad . \quad . \quad . \quad . \quad . \quad (52)$$

Furthermore the zonal velocity in the stratosphere has a meridional variation given by

$$\frac{dU^{II}}{dy} \simeq -\frac{f\Lambda^2}{\gamma g} T(z_T) \quad . \quad . \quad . \quad . \quad (53)$$

For a variable U^{II}, the left side of the steady-state perturbation form of the vorticity equation (1) becomes

$$U^{II}\frac{\partial \zeta^{II}}{\partial x} + \left(\beta - \frac{d^2 U^{II}}{dy^2}\right) v^{II} + \left(f - \frac{dU^{II}}{dy}\right) \nabla_h \cdot \mathbf{v}^{II}$$

However, it can be shown for the values of the mean properties of our model that $\beta \gg d^2 U^{II}/dy^2$ and $f \gg d U^{II}/dy$. With this approximation and taking $\chi = 0$ in the stratosphere, Eqs. (13) become for V_1^{II} and V_2^{II} :

$$\frac{d^2 V_{1,2}{}^{II}}{dz^2} - \frac{1}{H^{II}}\frac{dV_{1,2}{}^{II}}{dz} - a^{II^2}\left(\frac{U^{II} - U_c}{U^{II}}\right) V_{1,2}{}^{II} = 0 \quad . \quad . \quad (54)$$

where

$$\left.\begin{aligned} &U_c = U_c{}^{*II}, \quad U^{II} = U^I(z_T) \\ &a^{II^2} = (\mu^2 + k^2)\nu^{II^2}/f^2, \quad \nu^{II^2} = g\gamma_d/\overline{T}^{II} \\ &H^{II} = R\overline{T}^{II}/g, \quad \overline{T}^{II} = T^{II} = T^I(z_T) \end{aligned}\right\} \quad . \quad . \quad (55)$$

Since the coefficients are constant, if we require that $v \to 0$ as $z \to \infty$, the solutions for V_1^{II} and V_2^{II} are :

$$\left.\begin{aligned} &V_{1,2}{}^{II} = M e^{mz} \\ &m = \frac{1}{2H^{II}} - \left(\frac{1}{4H^{II^2}} + a^{II^2}\frac{U^{II} - U_c}{U^{II}}\right)^{\frac{1}{2}} < 0 \end{aligned}\right\} \quad . \quad . \quad (56)$$

The constant M is determined by the condition of continuity at the tropopause : $V_{1,2}{}^I = V_{1,2}{}^{II}$.

By considering the thermodynamic energy equation on either side of the tropopause and requiring continuity in U, v and w, we have

$$\frac{\partial v^I}{\partial z} - \left(\frac{\nu^I}{\nu^{II}}\right)^2 \frac{\partial v^{II}}{\partial z} = \frac{\Lambda v}{U}, \quad z = z_T \quad . \quad . \quad . \quad . \quad (57)$$

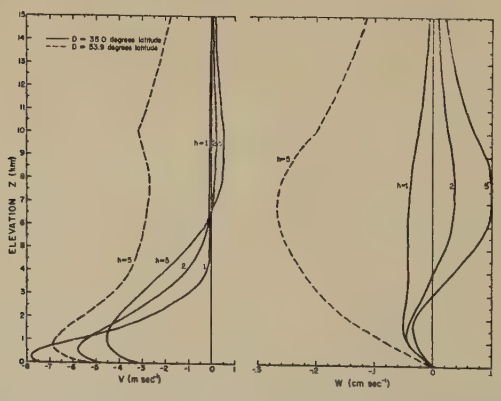

Figure 4. Vertical distribution of perturbation meridional component of velocity v, and vertical component w at maximum of a heat source for different vertical distributions of heating given by h. Stable stratosphere taken into account. Friction is not included. $L = 160°$ longitude. Solid lines $D = 35°$ latitude ($\equiv D_1$). Dashed lines $D = 53{\cdot}9°$ latitude ($\equiv D_2$).

With (12) we have for V_1 and for V_2

$$\frac{1}{V_{1,2}{}^{\mathrm{I}}}\frac{dV_{1,2}{}^{\mathrm{I}}}{dz} - \left(\frac{\nu^{\mathrm{I}}}{\nu^{\mathrm{II}}}\right)^2 \frac{1}{V^{\mathrm{II}}}\frac{dV_{1,2}{}^{\mathrm{II}}}{dz} = \frac{\Lambda}{U}, \quad z = z_T \qquad . \qquad . \qquad (58)$$

Using Eqs. (19) and (56) for the lower and upper fluids, respectively, we have for ϕ_1 and ϕ_2 that

$$\frac{\phi_{1,2}{}'(\xi_T)}{\phi_{1,2}(\xi_T)} = \lambda(\xi_T) + \frac{m}{2a^{*\mathrm{I}}}\left(\frac{\nu^{\mathrm{I}}}{\nu^{\mathrm{II}}}\right)^2 \equiv \lambda^*(\xi_T) \qquad . \qquad . \qquad . \qquad (59)$$

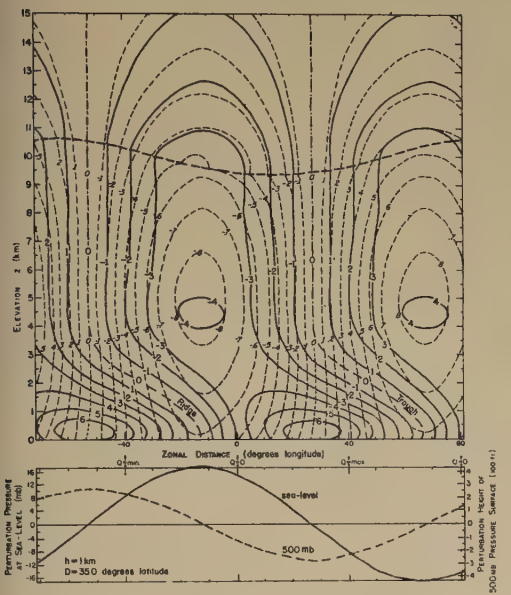

Figure 5 (a)

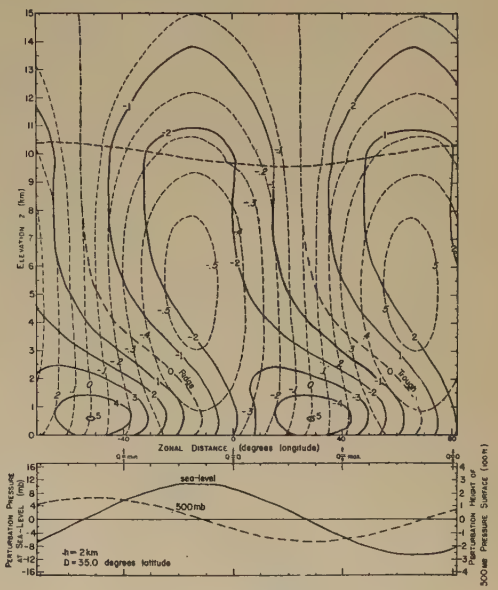

Figure 5 (b)

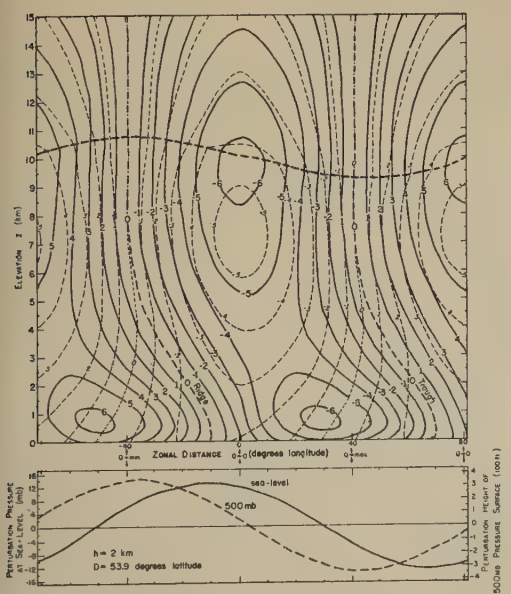

Figure 5 (c)

Figure 5. General baroclinic model with surface friction, at latitude of maximum heating and cooling. Stable stratosphere taken into account. Upper part : zonal section of perturbation meridional velocity v (solid lines) in m sec^{-1} and perturbation vertical velocity w (dashed lines) in cm sec^{-1}. Lower part : zonal profile of perturbation pressure at sea level (solid lines) and perturbation height of 500-mb pressure surface (dashed lines).

where $\lambda(\xi_T)$ has been defined in (23):

$$\lambda(\xi_T) = \tfrac{1}{2} - \frac{1}{4a^{*\mathrm{I}} H^{*\mathrm{I}}} + \frac{1}{\xi_T} \quad \text{and} \quad \xi = 2\, a^{*\mathrm{I}}\, U^{\mathrm{I}}/\Lambda \quad . \qquad (60)$$

By the condition of continuity of the velocity components at the tropopause and the thermodynamic energy equation for the stratosphere we have upon using (56):

$$W^{\mathrm{I}} = W^{\mathrm{II}} = -fm\, UV^{\mathrm{I}}\, (\nu^{\mathrm{II}})^{-2} \quad \text{at} \quad z = z_T \quad . \qquad (61)$$

The solutions for this model may be found in exactly the same manner as for the model with the rigid top – we need only to replace condition (22) by (59). The results in absence of friction are given in Fig. 4 and those with surface friction are given in Figs. 5 (a), (b), and (c).

7. Resonance

For the cases of the narrow lateral extent ($D = D_1$: $\mu^2 + k^2 = 2{\cdot}86 \times 10^{-6}\ \mathrm{km}^{-2}$) we find that when surface friction is taken into account the influence of the stratosphere on the tropospheric motions is negligible below 6 km while the amplitudes in the upper troposphere are reduced by no more than a factor of two. On the other hand, for the case with large lateral width ($D = D_2$: $\mu^2 + k^2 = 1{\cdot}35 \times 10^{-6}\ \mathrm{km}^{-2}$) the reduction at the tropopause is by a factor of three. The explanation lies in the fact that the latter case is closer to the non-frictional resonant wave number. Although infinite amplitudes are prevented by the inclusion of dissipative forces, large but bounded disturbances may still occur in the near vicinity of the non-frictional resonant wave number. We have shown that in the advective model with a rigid top, resonance occurs when $U_c = \overline{U^2}/\overline{U}$, that, is when $\mu^2 + k^2 = 1{\cdot}08 \times 10^{-6}\ \mathrm{km}^{-2}$. We can also determine the critical wave number for the general baroclinic model with little difficulty. Considering the limit when $\sigma \to 0$ in Eqs. (26), we find that

$$\left.\begin{aligned} E_1 &= E_2 = 0 \\ B(\xi_0) &= -\frac{C_3\,\kappa(\xi_0) - C_1\,[\kappa(\xi_T) + C_4\,C_6 - C_3\,C_5]}{C_1\,C_4 - C_2\,C_3} \\ A(\xi_0) &= \frac{C_4\,\kappa(\xi_0) - C_2\,[\kappa(\xi_T) + C_4\,C_6 - C_3\,C_5]}{C_1\,C_4 - C_2\,C_3} \end{aligned}\right\} \quad . \qquad (62)$$

Thus $A(\xi_0)$ and $B(\xi_0)$ become infinite when the denominator vanishes, i.e., when $C_1\,C_4 = C_2\,C_3$. This condition may be written in terms of the confluent hypergeometric functions with the aid of (34):

$$\frac{[\psi_1'(\xi_T)\,\psi_2'(\xi_0) - \psi_1'(\xi_0)\,\psi_2'(\xi_T)] - \lambda(\xi_0)\,[\psi_1'(\xi_T)\,\psi_2(\xi_0) - \psi_2'(\xi_T)\,\psi_1(\xi_0)]}{[\psi_2'(\xi_0)\,\psi_1(\xi_T) - \psi_1'(\xi_0)\,\psi_2(\xi_T)] - \lambda(\xi_0)\,[\psi_1(\xi_T)\,\psi_2(\xi_0) - \psi_1(\xi_0)\,\psi_2(\xi_T)]} = \lambda(\xi_T) \quad . \qquad (63)$$

For the model with a stratosphere one need only replace the right side of (63) with $\lambda^*(\xi_T)$. Using Charney's tables (1947), the left- and right-hand sides may be plotted as functions of r. The intersection indicates the value of r for which the above equation is satisfied. The result is that resonance occurs at $\mu^2 + k^2 = 1{\cdot}22 \times 10^{-6}\ \mathrm{km}^{-2}$ with a rigid top and at $1{\cdot}05 \times 10^{-6}\ \mathrm{km}^{-2}$ when there is an isothermal upper layer. This means that the influence of the stratosphere is to remove the model further from resonance than if there were a rigid top, all other properties of the models being the same.

Although the advective model with friction included cannot be expected to give reliable quantitative results, some qualitative features are of interest. One can introduce friction into the advective model in exactly the same manner as into the more general baroclinic model. This is done by using (15) for the lower boundary condition in the vertical integration of (35). Since the mathematical development is straightforward it is

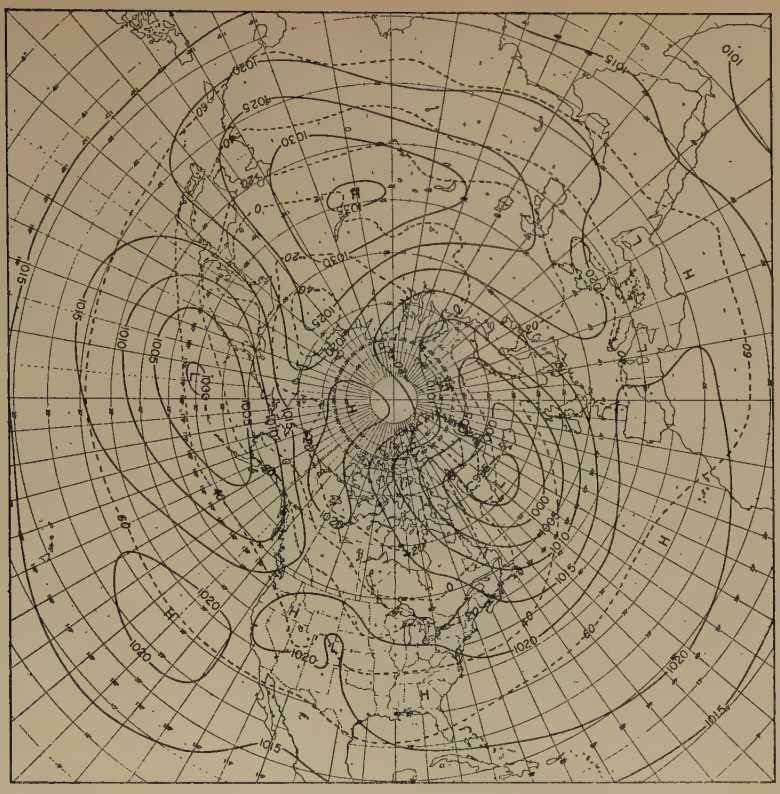

Figure 6 (a). January.

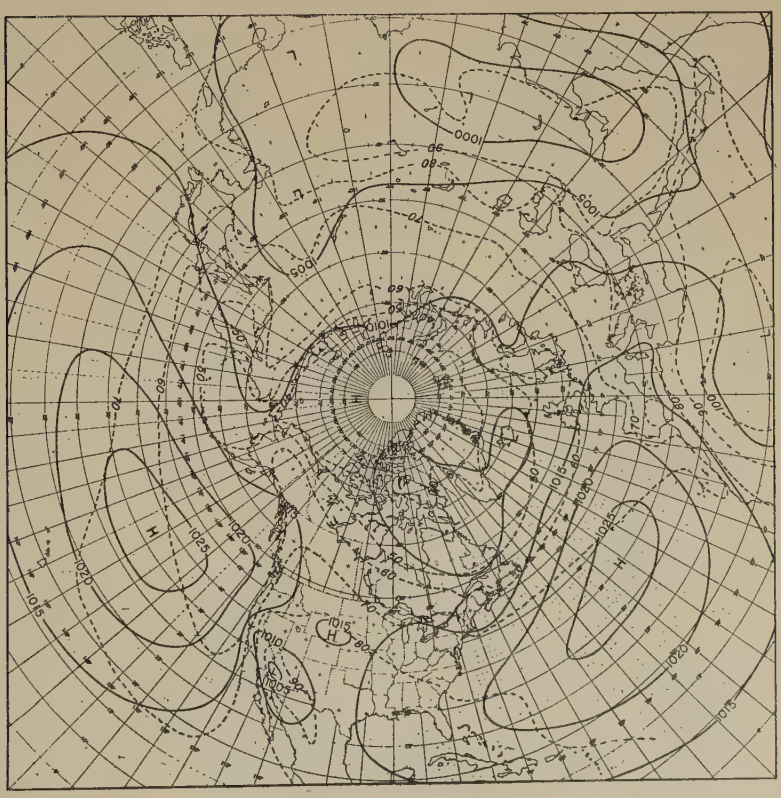

Figure 6 (b). July.

Figure 6. Normal monthly sea-level pressure (solid lines) in mb after United States Weather Bureau (1952) and temperature (dashed lines) in °F after Haurwitz and Austin (1944).

not reproduced here. By the condition for resonance for the advective model one sees that resonance may be reached either by reducing $\mu^2 + k^2$ or by reducing the zonal velocity. When this occurs the character of the stationary disturbance changes markedly. For instance, when $\mu^2 + k^2$ is taken to be $0{\cdot}90 \times 10^{-6}$ km^{-2} (for $L = 160°$ longitude this corresponds to $D \simeq 70°$ latitude), $U(z)$ is kept the same as before, and $h = 2$, the sea-level trough is located about 30°W of the heating minimum (cold source), and slopes *eastward* with height, so that at 500 mb the trough is 30° E of the heating minimum. The sea-level ridge is about 50°E of the heating minimum and at 500 mb is 110°E of the minimum*.

8. The observed normals in the light of the theoretical results

In testing the validity of the model on the basis of observation, one can compare only the orders of magnitude and general spatial distribution of the disturbances in relation to the field of heating. In the discussion which follows we shall refer to the maps of normal seasonal heating (Figs. 1 (a) and (b)) and to the maps of normal sea-level pressure and temperature (Figs. 6 (a) and (b)). The sea-level pressure is taken from tentative copies of new normals compiled by the United States Weather Bureau (1952) which were kindly made available before their publication. The normal sea-level temperatures were taken from the book *Climatology* by Haurwitz and Austin (1944). Although the 500-mb normals are not shown here, the reader is referred to the previously mentioned Weather Bureau publication. In order to facilitate the discussion, zonal profiles of the sea-level pressure and the height of the 500-mb surface (Fig. 7) are included. These are presented as deviations from the zonal average in a belt 20° latitude wide centred at 45°N in winter and at 50°N in summer. The continental elevations are also indicated schematically. The information on non-adiabatic heating contained in Fig. 1 leaves much to be desired regarding the location of the primary sources and sinks over the continents. However, one may deduce their locations from the geostrophic temperature advection at the surface provided there are no large changes in the continental elevations. The locations of primary extremes of heating determined from Fig. 1 and, where permissible, from surface temperature advection are also shown schematically in Fig. 7.

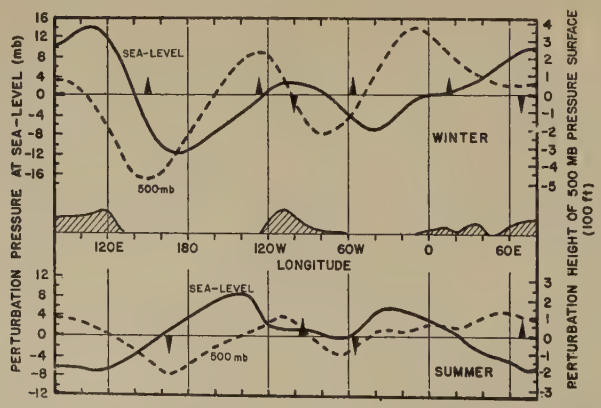

Figure 7. Zonal profiles of normal perturbation pressure at sea level (solid line) and normal perturbation height of the 500-mb pressure surface (dashed line). Continental elevations are indicated schematically by cross-hatched area. Positions of observed relative heat sources and sinks indicated by spires pointed upward and downward respectively. Upper part : winter profiles averaged over 20° latitude centred at 45°N. Lower part : summer profiles averaged over 20° latitude centred at 50°N.

* This is in substantial agreement with results obtained independently by B. Gilchrist who has made a similar investigation using a '$2\frac{1}{2}$ dimensional' baroclinic model. These results were communicated privately to the writer by Dr. Gilchrist who is at present at the Institute for Advanced Study.

(a) *Winter*

At 500 mb, the January normal exhibits an extremely broad trough with a primary minimum at the longitude of the western Great Lakes and a slight indication of a secondary minimum approximately at Newfoundland. The existence of the secondary trough is not apparent from the averaged profile in Fig. 7. The western extension is quasi-barotropic in structure : this trough is in phase at all levels in the troposphere but has its minimum amplitude near the surface. On the other hand, the eastern component increases in intensity and shifts eastward as we go to lower elevations. At the surface this eastward extension is observed as the Icelandic low, which is far more intense (a perturbation amplitude of about 7 mb) than the surface trough at the western Great Lakes. The position and intensity of the western component is in agreement with the calculated position and intensity of the orographically produced barotropic trough (cf., Charney and Eliassen (1949) and Bolin (1950)), while the eastern extension appears to be a baroclinic trough generated by sensible heating off the North American east coast and by heat of condensation just inland. These sources are characterized by the narrower lateral width, D_1. The model with friction has a warm sea-level trough about 25°E of the heating maximum with perturbation amplitude of the order of 10 mb. At 500 mb, the computed trough is about 10°W of the position of maximum heating. This is a little further west than the eastern extension of the observed normal trough. The existence of a ridge at 500 mb in the eastern Atlantic has been explained by the calculations of Charney and Eliassen as part of a resonance wave initiated by the Rockies.

In an earlier section it was shown that the heating from the pronounced condensation maximum along the North American west coast north of 40°N is over-compensated by the adiabatic cooling caused by the forced ascent along the western slopes of the Rockies. Similarly the large wintertime sink over North America due to downward eddy conduction and nocturnal radiation is completely nullified by the heating due to the forced descent along the eastern slopes. This is verified by the observation that there is weak cold-air advection east of the Rockies. This may explain why the sea-level anticyclone is located in the extreme western part of the continent, to the west of the non-adiabatic cold source, and is rather poorly developed compared with the Asiatic high-pressure cell.

In the far western Pacific the position of maximum precipitation almost coincides with that of the maximum upward eddy flux of sensible heat from the warm Kuroshio current. The sea-level Aleutian low appears to have the correct location with respect to these heat sources. As predicted by theory it lies roughly 25°E of the heat sources. The Siberian surface high-pressure cell is without doubt generated by the cold source to the west, the location of which was deduced in Section 1 from surface warm air advection.

The calculations indicate that the lower the level of the sources or sinks the more rapidly the phase of the disturbance changes with height. Off east coasts in winter the sources are due to a combination of condensation and eddy conduction and thus have a maximum at about 2 km. On the other hand the wintertime sink in western Asia, which is due to downward eddy conduction and nocturnal radiation, occurs at a lower level. The change of phase with height of the Aleutian low is consequently much less rapid than that of the Siberian anticyclone.

A further result of theory is that for equal amounts of heating in a vertical column, low-level sources or sinks produce a larger surface disturbance than sources and sinks at moderate levels. This would imply that the total relative cooling over central Siberia could be of appreciably smaller magnitude than the relative heating in the western Pacific and yet produce a surface monsoonal high of the same relative magnitude as the low off the east Asiatic coast.

The condensation source in the Mediterranean Sea has dimensions which are

considerably smaller than those of the sources and sinks which we have hitherto considered. This, added to the fact that the positions of the Mediterranean heat source and of the sea-level pressure minimum coincide, leads one to believe that the controlling mechanism in this case is more of the type that generates large-scale sea-breezes.

(b) *Summer*

We shall now attempt to diagnose the changes observed in the summer normal flow pattern. Since we are interested only in the perturbations of the zonal motion, we must examine them at a higher latitude corresponding to the northward shift of the westerlies. In the summer hemisphere there are no large regions of strong absolute cooling such as are found in the winter over continents. This means that regions of heating of large magnitude in the summer have deviations from the zonal mean heating which are considerably less effective in disturbing the flow. On the other hand, there is a counteracting element. Although the zonal momentum is reduced by more than one-half at the tropopause level from winter to summer, the low-level reduction is not nearly so great. Since the influence of the relative sources and sinks is most affected by the magnitude of the mean velocities, say, in the lowest 3 km, one should expect some increase in the disturbance produced by a given amount of relative heating.

The wintertime maximum of upward flux of sensible heat from the Gulf Stream off the North American east coast (cf. Fig. 7) vanishes in the summertime. The relative minimum of heating in the western Atlantic is in agreement with the warm-air advection found there and its location is by theory consistent with the position of the weak sea-level ridge to the east. There is now an upward flux of sensible heat over the continent, and the precipitation maximum over Eastern United States is broader but appears at a lower latitude. The principal sea-level Atlantic trough is now located over the east coast of North America, and is considerably less intense than the corresponding winter trough (the Icelandic low). This would indicate that the reduction of the low-level zonal velocity in summer is not sufficient to counteract the decrease in the contrast between heating and cooling.

Both the heating theory and the theory of orographical influences predict a trough at 500 mb which is further to the west than the weak trough observed at the North American east coast. No explanation has yet been found for this discrepancy.

The dimensions of the summertime low-pressure area in south-western United States indicate that the motions here are governed by the same mechanism as the wintertime low over the Mediterranean. Although a condensation minimum occurs in this arid region, it is the location of a sensible-heat source in summer. It seems that the traditional explanation for the formation of this 'heat low' is consistent with scale considerations.

The maximum warm-air advection just off the Asiatic east coast indicates a relative cold source. The wintertime sea-level Aleutian low has virtually disappeared and a trough now exists at about 120°E, approximately 50°W of the relative cold source. The 700-mb and 500-mb normals show that this trough slopes to the east with increasing height and at 500 mb appears at the longitude of the cold source. One would expect a weak 500-mb trough induced by the flow of the summer westerlies over the relatively low mountains to the north of the Tibetan Plateau to appear just to the west of this longitude (Bolin 1950). The advective model with friction yields an eastward slope of pressure axes with height if either $\mu^2 + k^2$ or the zonal velocity is reduced sufficiently. Apparently this has occurred over the Pacific in summer. According to the advective model, the 500-mb thermally-produced trough should be located to the east of the longitude of maximum relative cooling. It thus appears that although the theoretical thermal and orographical disturbances are weak and somewhat out of phase with each other at 500 mb, together they can explain the observed trough. Furthermore, the

position of the sea-level ridge in the eastern Pacific relative to the cold source in the western Pacific is also in general agreement with the theoretical results, providing that resonance has been crossed over.

The strong winter anticyclone over eastern Asia is replaced by a general low-pressure area. This is probably a consequence of the reversal of the direction of eddy heat flux and the elimination of long-wave radiational cooling in the lowest layers. The sink has been replaced by a source which, according to our theory, produces a low to the east at sea level.

(c) *Conclusions*

It appears that the mechanism proposed is capable of accounting for the gross features of the observed sea-level normals at middle and high latitudes. Further, the influence of large-scale asymmetries of non-adiabatic heating and cooling on mid-tropospheric flow is calculated to be of the same magnitude as the influence of broad mountain areas. Charney and Eliassen (1949) and Bolin (1950) have concluded that the orographic influence on 500-mb motions is of primary importance whereas Sutcliffe (1951) in a qualitative argument contends that the thermal influence dominates. It must be said that the present quantitative study does not conclusively support or refute either point of view. If one is willing to discount the near-resonance calculations, the computations do indicate that the thermal effect becomes relatively less important as we go to higher levels.

The inclusion of surface friction appears to be necessary in order to avoid resonance. It also yields more realistic motions. In order to obtain an undistorted estimate of the magnitude of the disturbances in the upper third of the troposphere, it appears that the influence of the stratosphere must be taken into account. The large-scale horizontal eddy transport of heat or momentum does not appear to be a deciding factor in determining the quasi-stationary flow, but may be of primary importance for the transient states.

The idealizations regarding the distribution of heating utilized in this investigation were dictated by the lack of information concerning this quantity. Current knowledge does not warrant the use of the observed field for a more precise analysis of the type carried out by Charney and Eliassen for orographically produced perturbations. It is felt, therefore, that with the availability of better data, this aspect of the problem should be pursued further.

Since the normal three-dimensional mass field is known empirically better than the heating field, a companion study to the present one suggests itself. It is possible to test the proposed heating model by deducing the composite three-dimensional field of non-adiabatic heating dynamically from the observed normal maps by means of the non-linear equations. The vorticity equation

$$\mathbf{v}_h \cdot \nabla_h (\zeta + f) = \frac{f}{\rho} \frac{\partial (\rho w)}{\partial z}$$

may be applied to the normals if the eddy stresses and the time-dependent term may be ignored. For a particular monthly normal one can compute the three-dimensional distribution of the horizontal advection of absolute vorticity geostrophically. From this distribution one may determine the difference between the vertical momentum at any level z and the top of the friction layer z_0, thus

$$\rho w - (\rho w)_{z=z_0} = -\frac{1}{f} \int_{z_0}^{z} \rho \mathbf{v}_h \cdot \nabla_h (\zeta + f)\, dz$$

A knowledge of the large-scale orographical features and an estimate of the vertical mean geostrophic wind in the friction layer enables one to calculate the portion of $(\rho w)_{z=z_0}$ resulting from continental elevations. That part of $(\rho w)_{z=z_0}$ due to frictional effects may be estimated by means of some simple frictional law such as the one used in this

paper. One now has adequate information to deduce the three-dimensional field of vertical motion dynamically. The large-scale distribution of Q is determined from the steady-state version of the thermodynamic energy equation (3), since now all the quantities besides w can be measured directly from the normals. One of the virtues of such an approach is that the orographic influence on the normals is implicitly taken into account.

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The evaluation of infra-red radiative fluxes due to atmospheric water vapour*

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Summary

An empirical technique is developed for obtaining the flux transmission, over a narrow spectral interval, of an assembly of layers of varying pressure and temperature. Although verified only for a regular spectrum of the type proposed by Elsasser (1942), the technique should be valid for any type of spectrum. Using this technique as a check, an empirical method of sufficient accuracy is developed for evaluating the integrated flux transmission of an assembly of layers. This latter method enables the radiation-chart procedures, advocated by Elsasser (1942), Robinson (1947), and Yamamoto (1952), to be tested for simply-stratified atmospheric models. Such tests reveal that the effects of temperature are inadequately taken into account. A principle advocated by Cowling (1950b) is developed into a radiation-chart procedure, in which the absorbing mass of each individual layer is corrected to a standard temperature and pressure. The coordinates of such a radiation chart are presented, but unfortunately adequate experimental data for the construction of such a chart are not yet available.

1. Introduction

Atmospheric radiation studies are in general directed toward one or both of two goals : (a) prediction of the radiative fluxes at the ground, at outer space, and at cloud bases and tops, and (b) the prediction of the radiative flux divergence, which is proportional to the local cooling rate, at various levels. Such studies invariably prove complex, for a number of reasons. Cowling (1950a) has demonstrated the significant effects of pressure and temperature on absorption and emission. Moreover, although water vapour is the principal absorber of terrestrial radiation, over limited spectral ranges minor atmospheric constituents, such as carbon dioxide and ozone, also become important. Deacon (1950) and Yamamoto (1952) have developed graphical techniques which take into account the effects of both water vapour and carbon dioxide on radiative transfer. Finally, there is the problem of accumulating the data required for numerical computations. The difficulties of making exact theoretical calculations have been pointed out by Plass and Warner (1952a, b). Fortunately, it appears possible to utilize laboratory data to develop suitable transmission functions, in a manner indicated by Plass (1952).

In this paper, attention will be focused on the first of the above problems. In particular, techniques will be developed for the evaluation of radiative fluxes at any level in the atmosphere. The effect of carbon dioxide will not be considered at this time, but it is extremely unlikely that its consideration would invalidate the general techniques to be described. Flux divergence will not be treated directly, except for finite layers. Finally, no attempt will be made to suggest appropriate numerical values for transmission data, and all actual values utilized must be regarded as only approximate. Such values are used only to evaluate the possible errors resulting from the application of various flux-computation techniques.

2. The basic problem

Following Elsasser (1942), the total radiative flux, F, crossing a given level from above, can be obtained from the differential equation of radiative transfer in the following

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form if only a single absorber is present :

$$F = -\int_0^{\infty} d\nu \int_0^{m_\infty} f_b \frac{d}{dm}\{2\,Ei_3(\bar{k}_\nu\, m)\}\, dm \quad . \quad . \quad . \quad (1)$$

where ν represents frequency, f_b the monochromatic black-body flux at the level specified by m, the absorbing mass between the reference level and an arbitrary level, m_∞ the total absorbing mass above the reference level, Ei_3 the third exponential integral, and $\bar{k}_\nu$ represents the mean absorption coefficient at frequency ν for the layer of absorbing mass m.

Since $\bar{k}_\nu$ varies irregularly and rapidly with frequency, it is necessary to average out these extreme fluctuations by evaluating (1) over a series of relatively narrow spectral intervals, for each of which f_b could be considered as a constant. The total flux would then be given by the following summation :

$$F = -\Sigma_\nu \int_0^{m_\infty} f_b \frac{d}{dm}\left[\int_\nu^{\nu+\Delta\nu} 2\,Ei_3(\bar{k}_\nu\, m)\, d\nu\right] dm \quad . \quad . \quad (2)$$

The quantity enclosed within the square brackets in (2) can be recognized as the mean value, over the spectral interval $\Delta\nu$, of a transmission function, multiplied by $\Delta\nu$. This transmission function, which will be denoted by τ_f, expresses the fraction of a black-body flux (i.e., hemispheric radiation) which is transmitted by a given layer over a specific spectral interval. It has been variously named the flux or slab transmissivity or transmission function (Elsasser and King 1952). Here it will be referred to simply as the flux transmission. It will also be necessary to refer to a similar type of transmission function, denoted by τ_I, which applies to parallel-beam radiation. This quantity is usually termed the transmission, or the beam transmissivity or transmission function. To avoid ambiguity and to be consistent with the definition of flux transmission (as well as with the symbols, which are standard), τ_I will be referred to here as the intensity transmission.

To evaluate these transmission functions, it is necessary to express $\bar{k}_\nu$ (or k_ν, if variations with m are not specifically considered) as an analytical function of ν in the spectral range considered. Assuming a constant mixing ratio with height for the absorbing gas, and a spectral model first proposed by Elsasser (1942) (equispaced spectral lines of equal intensity), Kaplan (1952a) and Elsasser and King (1952) have obtained the corresponding transmission functions. Kaplan (1952b) also developed a relation for spectral lines of variable intensity and spacing, which could only be integrated numerically. Matossi, Mayer and Rauscher (1946) considered a transmission function for random lines, and Goody (1952) solved such a problem for a layer at fixed pressure and temperature.

These transmission functions have almost invariably been based on the Lorentz relation for the absorption coefficient, k_ν, which for a single line is

$$k_\nu = S\,\alpha\left[\pi\{(\nu-\nu_0)^2+\alpha^2\}\right]^{-1} \quad . \quad . \quad . \quad . \quad (3)$$

where S represents the total intensity of the line (a function only of temperature for a given line), α the half-width of the line (proportional to the total pressure), and ν_0 the central frequency of the line. The significance of the linear dependence of half-width on pressure for radiative heat transfer has been demonstrated by Strong and Plass (1950).

It will be noted that the Lorentz relation (3) corresponds to a symmetric absorption line. Recent theoretical studies suggest, however, that except in the immediate vicinity of the line centre the line is asymmetric, and Plass and Warner (1952a) have shown that this may have an appreciable effect on flux divergence. This fact suggests that the accurate computation of the radiative flux by Eq. (2) would be very difficult. Furthermore,

it is necessary to introduce atmospheric sounding data into the problem prior to the summation over all frequencies, i.e., into the computation for each narrow spectral interval. Thus, for normal or routine use, there is a definite need for a different technique, in which wavelength variations are taken into account in advance, and only an integration or summation in the vertical through the atmosphere is required.

Introducing the integrated flux transmission, ϕ, defined by

$$F_b \phi = \Sigma_\nu f_b \tau_f \Delta\nu \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

where F_b represents the total black-body flux (also integrated over all frequencies), it can simply be shown that (2) becomes

$$F = -\int_0^{m_\infty} F_b \left\{\frac{\partial\phi\,(m, p, T; T_b)}{\partial m}\right\}_{T_b} dm \quad . \quad . \quad (5)$$

Here the integrated flux transmission, ϕ, is indicated to be a function of absorbing mass (m), pressure (p), and temperature (T), for the layer from $m = 0$ to $m = m$, and of the temperature T_b at the level m, since ϕ expresses the transmission by the layer of the black-body radiative flux, $F_b = \sigma T_b^4$.

The basic problem to be solved is brought into clear focus by Eq. (5). In order to evaluate (5) by graphical or analytical means, it is necessary to know the fraction of the radiation emitted by a black body at T_b, which is transmitted by an atmosphere of variable composition, pressure and temperature. It will be assumed that transmissions for layers of fixed pressure and temperature are known, either from experiment or from theoretical calculations.

In order to test the validity of various possible techniques, it is necessary to make use of computed integrated flux transmissions for various pressures, temperatures, radiating temperatures and absorbing masses. It is also necessary to have available values, for a narrow spectral range, of flux transmission (τ_f) and intensity transmission (τ_I). It was decided to make use of Elsasser's (1942) simplified model, assuming equidistant spectral lines with intensities either constant or randomly distributed (equivalent assumptions). This model yields transmission functions which depend on only two non-dimensional parameters, lm and β, defined as

$$lm = \beta S m/d, \quad \beta = 2\pi\alpha/d \quad . \quad . \quad . \quad . \quad (6)$$

where d represents the mean spacing of the spectral lines, and l is Elsasser's generalized absorption coefficient. Tables of τ_I and τ_f have been presented by Godson (1952).

Between absorption bands, the absorption coefficient changes only slowly with frequency since the chief contributions are from distant lines. In such spectral regions, a mean value of k_ν can be used, in conjunction with monochromatic transmission functions. For the computation of integrated flux transmissions, values of l, β, and k_ν, as tabulated by Yamamoto and Onishi (1949), were used. Although the absolute accuracy of the tabulated values is undoubtedly not high, comparisons between the various sets of integrated flux transmissions (Godson 1952), for the same values of absorbing mass or of transmission, should give reasonably reliable indications of the effects of pressure, temperature, and black-body temperature on integrated flux transmission. The chief inaccuracies, apart from the assumed model, arise from doubtful line intensities for the fundamental vibration-rotation band, near 6 μ, as recognized by Yamamoto (1952), and from the incorrect absorption coefficients predicted by the Lorentz relation between absorption bands, as pointed out by Plass and Warner (1952b). However, King (1952) has demonstrated that these spectral regions are relatively unimportant in studies of radiative transfer above the lower troposphere.

3. Flux transmission for an assembly of layers

In order to develop a suitable technique for obtaining the integrated flux transmission for a layer of variable pressure, temperature and composition, it is helpful to consider first the intensity and flux transmissions of an assembly of layers, each with specific values of pressure and temperature. At a single frequency, the net intensity transmission for a number of layers of varying pressure and temperature is simply the product of the separate intensity transmissions of each layer. This will apply also to spectral ranges between bands. Thus,

$$\tau_I = \Pi_n (\tau_I)_n = \Pi_n \exp (-k_\nu m)_n = \exp \Sigma_n (-k_\nu m)_n \quad . \quad . \quad (7)$$

Similarly, the flux transmission will be $2\, Ei_3\, \Sigma_n (k_\nu m)_n$.

In the general case, the problem (assuming the Elsasser model) is to determine those values of lm and β for the assembly of n layers which give the net flux transmission. Analytic solutions of this problem can be obtained only in the limiting cases of $lm/\beta^2 \to 0$ and $lm/\beta^2 \to \infty$. In the former case, an approximate solution is

$$lm = \Sigma_n (lm)_n, \quad \beta = \Sigma_n (lm)_n / \Sigma_n (lm/\beta)_n \quad . \quad . \quad . \quad (8)$$

In the case of large values of lm/β^2, an approximate solution is

$$lm = \Sigma_n (lm)_n,$$
$$\beta^2 = \Sigma_n (lm\beta^2)_n / lm + \Sigma_n (lm\beta^4)_n / 2\, lm - \tfrac{1}{2} [\Sigma_n (lm\beta^2)_n / lm]^2 \quad . \quad (9)$$

lm is defined by identical relations in (8) and (9), and the two values of the effective β, while distinct, will not be very different except in unusual cases. Transmissions computed from (8) and (9) will thus be very similar and examples will show that the true value of τ_I always lies between these two limiting values. Numerical integration gives the actual values of τ_I and τ_f for the assembly of layers in the case of the Elsasser model.

If only the temperature and absorbing mass vary from layer to layer, it can simply be shown, for all m and β, since β is a function only of pressure (in this case a constant), that

$$\tau_f = \tau_f [\Sigma_n (lm)_n, \beta] \quad . \quad . \quad . \quad . \quad (10)$$

If the pressure varies from layer to layer, it is not possible, for example, to replace m_1, at T_1 and p_1, by an equivalent absorbing mass, m_2' say, at T_2 and p_2, which would give the same monochromatic transmission at all frequencies inside $\Delta\nu$. A value of m_2' can be chosen, however, to give the same average transmission across $\Delta\nu$, by using the relation

$$\tau_f (l_1 m_1, \beta_1) = \tau_f (l_2 m_2', \beta_2) \quad . \quad . \quad . \quad (11)$$

The transmission of $(m_2 + m_2')$, at T_2 and p_2, will not be exactly the same as the actual net transmission of the two layers, but should be a fair approximation.

The above technique could be extended to cover the general case of a large number of layers, with varying pressure and temperature. Computations show, however, that this technique gives differing results, depending on the order of transmission through the various layers, whereas from theory the net transmission should be quite independent of the order of stratification. Intuitively, the average of the transmissions obtained from this technique for different arrangements of the layers might be expected to be in the vicinity of the correct result. Computations for the three-layer problem showed that the average for all six possible orders of transmission agreed, within the probable error of computations, with the average for the two orders, up and down, i.e., decreasing and increasing β, since β is directly proportional to the pressure. Accordingly, this simple average transmission was utilized in subsequent computations.

A series of computations was performed of two- and three-layer transmissions, in order to investigate the order of magnitude of the differences by various techniques. The values assumed for each layer are given in Table 1, together with the limiting values of β, for the assembly of layers as computed from (8), denoted by β', and as computed from (9), denoted simply by β. *lm* for the assembly of layers is the same for both assumptions, as can be seen from (8) and (9).

TABLE 1. BASIC DATA FOR TWO- AND THREE-LAYER TRANSMISSIONS OVER A NARROW SPECTRAL INTERVAL

Case	*lm* for $\beta = 0{\cdot}5$	*lm* for $\beta = 0{\cdot}3$	*lm* for $\beta = 0{\cdot}1$	β : (9)	β' : (8)
A	0·50		0·36	0·396	0·187
B		0·42	0·36	0·232	0·156
C	0·50	0·18		0·458	0·425
D	0·50	0·014		0·496	0·491
E	0·50		0·012	0·495	0·457
F		0·42	0·012	0·296	0·284
G		0·014	0·36	0·115	0·103
H		0·18	0·012	0·286	0·267
I	0·0048	0·18		0·306	0·303
J		0·014	0·012	0·232	0·156
K		0·014	0·0036	0·267	0·213
L	0·0048		0·0036	0·399	0·184
M	0·50	0·18	0·012	0·455	0·402
N	0·50	0·014	0·012	0·491	0·451
O	0·50	0·014	0·0036	0·495	0·478
P	0·48	0·42	0·36	0·365	0·211
Q	0·60	0·216	0·024	0·453	0·389
R	0·50	0·18	0·02	0·453	0·389
S	0·16	0·14	0·12	0·365	0·211
T	0·02	0·0072	0·0008	0·453	0·389
U	0·0048	0·0042	0·0036	0·365	0·211

The results of 15 computations of intensity transmission are shown in Table 2. The second column gives τ_I (*lm*, β), using (9); the third column gives τ_I (*lm*, β'), using (8); the fourth column gives τ_I evaluated by numerical integration; the fifth column gives $\overline{\tau_I}$, evaluated by the suggested technique based on (11); the sixth column gives the average deviation (called $\overline{\Delta\tau_I}$) from the mean for the two transmissions averaged to give $\overline{\tau_I}$. These computations were not carried out for cases P to U, inclusive. All transmissions are expressed as decimal fractions.

A comparison of Tables 1 and 2 shows that the empirical technique ($\overline{\tau_I}$) gives satisfactory accuracy if the values of *lm* for the separate layers, which could be considered to be at 1,000 mb, 600 mb, and 200 mb respectively, are of the same order (cases A, B, C, J, K, and L). The agreement is also good if *lm* increases markedly while β decreases (cases G and I), or if *lm* decreases markedly for less than a 50 per cent reduction in β (case D). Significant disagreement exists only if *lm* decreases markedly while β decreases (excluding case D mentioned above). In these cases : E, F, H, M, N, and O, $\overline{\tau_I}$ is less than τ_I, by approximately 60 per cent of $\overline{\Delta\tau_I}$. Although these cases correspond to variations of absorbing mass with pressure for deep atmospheric layers, only over limited spectral ranges would errors of the magnitudes found be experienced. For spectral intervals exhibiting very weak or very strong absorption, the errors would be much smaller.

TABLE 2. NET INTENSITY TRANSMISSIONS FOR TWO- AND THREE-LAYER CASES

Case	$\tau_I\,(lm, \beta)$: (9)	$\tau_I\,(lm, \beta')$: (8)	τ_I	$\overline{\tau_I}$	$\overline{\Delta\tau_I}$
A	0·369	0·358	0·367	0·366	0·010
B	0·383	0·380	0·382	0·382	0·004
C	0·435	0·431	0·433	0·433	0·006
D	0·508	0·508	0·508	0·507	0·002
E	0·509	0·504	0·508	0·503	0·007
F	0·524	0·523	0·524	0·521	0·004
G	0·544	0·543	0·543	0·544	0·003
H	0·682	0·679	0·681	0·677	0·007
I	0·692	0·691	0·691	0·691	0·001
J	0·910	0·893	0·895	0·896	0·007
K	0·940	0·931	0·933	0·934	0·002
L	0·980	0·960	0·961	0·962	0·000
M	0·429	0·423	0·428	0·422	0·011
N	0·502	0·496	0·501	0·496	0·009
O	0·506	0·504	0·506	0·503	0·005

The results of 21 computations of flux transmissions are shown in Table 3. The second column gives $\tau_f\,(lm, \beta)$, using (9); the third column gives $\tau_f\,(lm, \beta')$, using (8); the fourth column gives τ_f as evaluated by numerical integration; the fifth column gives $\overline{\tau_f}$, evaluated by the proposed technique based on (11); the sixth column gives the average deviation $\overline{\Delta\tau_f}$, from the mean for the two transmissions averaged to give $\overline{\tau_f}$. All transmissions are expressed as decimal fractions, and may be in error by $\pm 0{\cdot}001$.

TABLE 3. NET FLUX TRANSMISSIONS FOR TWO- AND THREE-LAYER CASES

Case	$\tau_f\,(lm, \beta)$: (9)	$\tau_f\,(lm, \beta')$: (8)	τ_f	$\overline{\tau_f}$	$\overline{\Delta\tau_f}$
A	0·255	0·246	0·253	0·253	0·006
B	0·270	0·267	0·269	0·269	0·002
C	0·314	0·311	0·313	0·312	0·006
D	0·387	0·386	0·385	0·385	0·002
E	0·388	0·384	0·386	0·382	0·005
F	0·410	0·409	0·409	0·407	0·002
G	0·434	0·433	0·433	0·435	0·002
H	0·585	0·583	0·583	0·581	0·005
I	0·595	0·594	0·593	0·595	0·001
J	0·860	0·844	0·850	0·852	0·010
K	0·903	0·891	0·895	0·896	0·006
L	0·961	0·934	0·937	0·938	0·003
M	0·310	0·306	0·308	0·306	0·007
N	0·380	0·376	0·378	0·375	0·006
O	0·385	0·383	0·383	0·381	0·004
P	0·171	0·167	0·170	0·170	0·004
Q	0·264	0·260	0·263	0·261	0·005
R	0·307	0·302	0·306	0·303	0·008
S	0·422	0·412	0·418	0·421	0·011
T	0·899	0·889	0·891	0·891	0·004
U	0·941	0·915	0·919	0·920	0·004

A comparison of Tables 1, 2, and 3 reveals that the empirical technique for flux transmissions is slightly more accurate than for intensity transmissions. For lm values of the same order, the agreement is good, except for cases of high transmission, when apparently $\overline{\tau_f}$ is too high (cases J, K, and U). $\overline{\tau_f}$ is, as was $\overline{\tau_I}$, too low if lm decreases markedly as β decreases, except for the case of high transmission (T), when compensation occurs.

From (6) it follows that l is proportional to the total pressure. Thus, in terms of l at a standard pressure of p_0,

$$lm = l_0 \, m \, p/p_0 = l_0 \, m_p \quad . \quad . \quad . \quad . \quad (12)$$

where m_p is an absorbing mass corrected linearly for pressure. For a given narrow spectral interval, l_0 is of the same order over the atmospheric temperature range. Thus the error for a given spectral interval will be small, using the empirical $\overline{\tau_f}$ technique, if $l_0 \, m_p$ is of the same order for individual atmospheric layers. If $l_0 \, m_p$ decreases markedly as the layer mean pressure decreases, the errors will be a maximum, especially for a large value of β at the standard pressure (taken as one atmosphere). In such ranges, however, even thin layers give very small transmissions, greatly reducing the possible errors. Furthermore, even if $l_0 \, m_p$ decreases rapidly with the layer mean pressure, spectral regions with very small l_0 will produce errors that tend to offset those in regions of average l_0 values. Finally, in the spectral regions of weak absorption due to distant strong lines, the empirical technique, now in terms of the Ei_3 function, is identical with the theoretical procedure. As a result, when the entire spectrum is considered the resultant error will in general be very small.

4. Integrated flux transmissions for an assembly of layers

Using (4) and (11), the integrated flux transmission for the two-layer problem can be assumed to be given approximately by

$$2 F_b \, \phi = \Sigma \left[\tau_f (m_2 + m_2', p_2, T_2) + \tau_f (m_1 + m_1', p_1, T_1) \right] f_b \, \Delta\nu \quad . \quad (13)$$

where $$\tau_f (m_1, p_1, T_1) = \tau_f (m_2', p_2, T_2),$$

and $$\tau_f (m_2, p_2, T_2) = \tau_f (m_1', p_1, T_1).$$

From the considerations of the last paragraph of Section 3, it may be concluded that the error in ϕ, using (13), is generally relatively small compared to $\Sigma \, \overline{\Delta\tau_f} \, f_b \, \Delta\nu / F_b$, which will be denoted by $\overline{\Delta\phi}$: (13).

m_2' and m_1' in (13) will vary from one spectral range to another. In a somewhat similar manner, the monochromatic m_2' and m_1' vary across $\Delta\nu$, but an effective mean value may fortunately be chosen, so that on averaging the two path directions the net transmission is closely approximated. It might be anticipated, therefore, that an analogous technique could be employed for the integrated net transmission. This would lead to

$$2 \phi = \phi (m_2 + m_2', p_2, T_2 ; T_b) + \phi (m_1 + m_1', p_1, T_1 ; T_b) \quad . \quad . \quad (14)$$

The average deviation from the mean of the two values of ϕ on the right of (14) will be denoted by $\overline{\Delta\phi}$: (14).

The above technique could simply be extended for the multi-layer problem, by averaging the integrated net transmissions obtained by proceeding upwards and downwards through the assembly of layers. This method is illustrated on Fig. 1, for the three-layer problem. The curves ϕ_1, ϕ_2, and ϕ_3 are schematic integrated flux-transmission curves appropriate to the pressure and temperature of layers one, two, and three; all

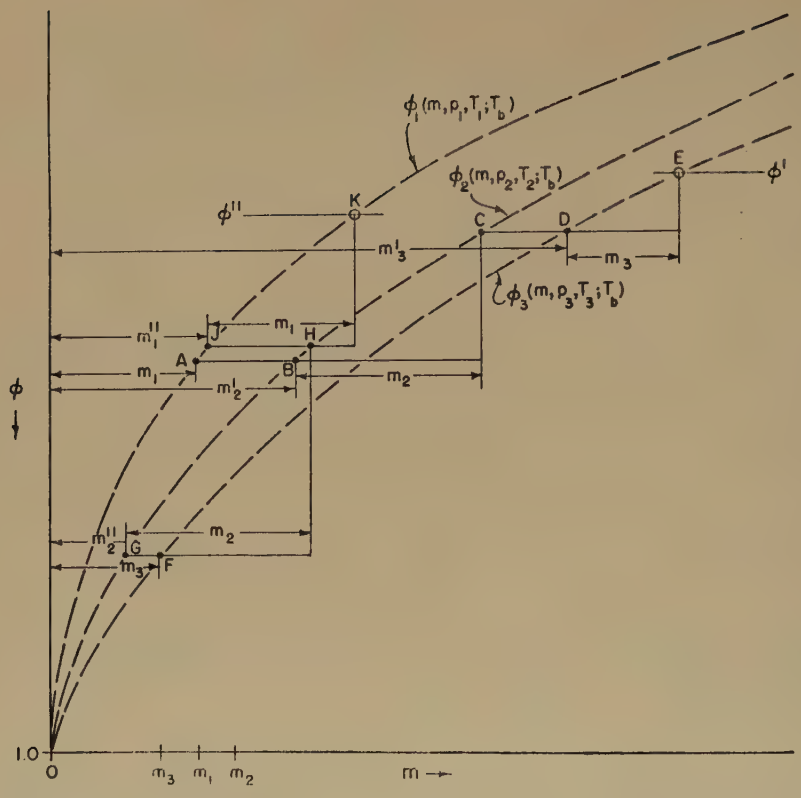

Figure 1. Schematic representation of procedure for evaluating the net integrated flux transmission for three layers.

apply to the same radiating temperature, T_b. The single primes refer to the order : 1, 2, 3; the double primes refer to the reverse order of layers. Point A represents $\phi_1(m_1)$; point B represents the same transmission on curve ϕ_2, and hence the abscissa of B is m_2'. Adding m_2 to m_2' gives, on curve ϕ_2, point C. Point D gives the same transmission on curve ϕ_3, and hence the abscissa at D is m_3'. Adding m_3 to m_3' gives, on curve ϕ_3, point E, which represents the net transmission, ϕ', for the upward path. In an exactly similar manner, ϕ'', at K, gives the net transmission for the downward path, i.e., for layers 3, 2, and 1, in that order. The assumption is then made that the average of ϕ' and ϕ'' represents the actual transmission of the three layers, which must, of course, be independent of the vertical order of the layers.

The empirical technique outlined above, based on an extension of (14), can be checked by computations based on (13), or on its extension if there are more than two layers. This was done for eight cases of the two-layer problem, and the results are presented in Table 4. For all cases, the following values were assumed :

$$p_1 = 1 \text{ atm}, \quad p_2 = 0{\cdot}5 \text{ atm}, \quad T_1 = 300°\text{K}, \quad T_2 = 220°\text{K};$$

for cases A to D, T_b was taken as 220°K; for cases E to H, T_b was taken as 300°K. The second and third columns give the values of m_p (in g cm^{-2}) assumed for the two layers. The third column gives the values of ϕ computed by the more complex method, using (13), and the fourth column presents $\overline{\Delta\phi}$: (13). The fifth column gives the values of ϕ computed by the simpler method, using (14), and the sixth column presents $\overline{\Delta\phi}$: (14). All transmissions are expressed as percentages of the black-body flux appropriate to T_b.

TABLE 4. COMPARISON OF COMPUTED NET INTEGRAL FLUX TRANSMISSIONS FOR TWO-LAYER CASES

Case	$(m_p)_1$	$(m_p)_2$	ϕ : (13)	$\overline{\Delta\phi}$: (13)	ϕ : (14)	$\overline{\Delta\phi}$: (14)
A	1·0	0·5	14·91	0·00	15·05	0·35
B	0·1	0·1	42·29	0·03	42·40	0·51
C	0·01	0·1	50·11	0·07	50·12	0·13
D	0·01	0·002	63·95	0·11	63·94	0·12
E	1·0	0·5	21·84	0·00	21·98	0·36
F	0·1	0·1	52·77	0·01	52·88	0·47
G	0·01	0·1	60·15	0·04	60·18	0·10
H	0·01	0·002	72·88	0·08	72·88	0·10

The two computational techniques illustrated in Table 4 apparently give almost identical results if the values of the pressure-adjusted absorbing mass (m_p) for the two layers are relatively dissimilar – cases C, D, G, and H. For cases C and G, ϕ : (13) is probably correct to within 0·01 to 0·02 per cent, judging from cases G and I in Tables 1 and 3. For cases D and H, ϕ : (13) is probably too small by about 0·02 or 0·03 per cent (of F_b), so that in all four cases the error in ϕ : (14) is of no practical significance.

If the values of the pressure-adjusted absorbing mass, m_p, for the two layers are relatively similar, the simpler technique gives larger net transmissions, by 0·12 per cent (of F_b) on the average (cases A, B, E, and F). For these cases ϕ : (13) is undoubtedly correct to within 0·01 per cent, at least with reference to the model spectrum assumed in these discussions. However, in the examples of Table 4 the pressures and temperatures for the two layers were deliberately chosen to have relatively extreme values – 1·0 atm and 0·5 atm, and 300°K and 220°K, respectively. For two layers with pressure and temperature values more nearly similar, the errors with the suggested technique would be definitely smaller. Thus, for example, in evaluating the transmission of the surface black-body flux by the entire atmosphere, for the first few layers the values of absorbing mass would be of the same order, but so would the values of pressure and temperature. As the pressure and temperature vary more rapidly, between layers well above the surface, the pressure-adjusted absorbing mass of a layer becomes small relative to the effective net absorbing mass below that layer. Similarly, in computing the integrated net flux transmission downwards, there will be relatively few instances of layer-combinations resulting in appreciable errors, since an error of even 0·1 per cent of F_b would seldom be significant. The greatest contribution to the downward flux at the surface, moreover, comes from the lowest layers, and such fluxes should be relatively free of multiple-layer errors.

It may be concluded that the suggested technique, based on (14) and illustrated on Fig. 1, should prove to be of sufficient accuracy for studies of flux determination, provided that the spectral model adopted is a reasonable one. For analytical computations with such a technique, it would be necessary to use accurate integrated flux transmissions, and to incorporate the effects of carbon dioxide. Finally, since pressure and temperature vary continuously in the atmosphere, it is necessary to ascribe to a finite layer effective values of p, T, and T_b. In the case of emission, the value of T_b will depend on the direction of the emitted flux. Suitable techniques for the determination of these quantites have been developed by Godson (1952).

5. THE ACCURACY OF RADIATION-CHART PROCEDURES

The analytical technique outlined in Section 4 for flux computation would undoubtedly prove too tedious and complex for routine use. It is desirable, therefore, to investigate

the possibility of a graphical solution which would not be seriously in error. The results of Section 4 can be used as a check on the accuracy of such techniques, again employing the values of $\phi(m_p, p, T; T_b)$ computed by Godson (1952).

From (5), it can be seen that a graphical solution is feasible only if the integrated flux transmission of the layer from $m = 0$ to $m = m$ for black-body radiation at $T(m) = T_b$ can be expressed as a function of T_b and some measure of the total absorbing mass of the layer. It would also be possible to take into account the mean pressure for the layer m, but not the distribution of pressure and temperature with absorbing mass in that layer.

The effective absorbing mass can be represented as $\Sigma m(p)$, indicating a pressure correction for each individual layer. It is known from the studies of Cowling (1950a), Goody and Wormell (1951), and Kaplan (1952a) that for deep layers the pressure-corrected absorbing mass is simply the pressure-adjusted absorbing mass, m_p (i.e., mp/p_0). For thin layers this is no longer true, but a linear correction for pressure should not be seriously in error except for fluxes and flux divergences, at high levels, as will be demonstrated later in this section. Use of the square-root pressure correction at all levels, as previously advocated, has been demonstrated to be invalid, and classical computational techniques based on this assumption will not be considered here.

The basic postulate made by Elsasser (1942) for fluxes and Bruinenberg (1946) for flux divergences was that

$$\phi = \phi(m_p; T_b).$$

This will be denoted as technique E in the computations to be presented, and assumes that a constant pressure (p_0 = one atm) and temperature are adopted. 260°K was chosen for the constant temperature, so that the E technique can be expressed as

$$\phi(\Sigma m, p, T; T_b) = \phi(\Sigma m_p, 1{\cdot}0, 260; T_b) \quad . \quad . \quad (15)$$

The basic postulate made by Robinson (1947, 1950) for fluxes and D. L. Brooks (1950) for flux divergences was that

$$\phi = \phi(m_p)$$

This will be denoted as technique R in the computations to be presented, and requires constant values for p, T and T_b. The R technique was assumed to be

$$\phi(\Sigma m, p, T; T_b) = \phi(\Sigma m_p, 1{\cdot}0, 260; 260) \quad . \quad . \quad (16)$$

Yamamoto (1952) evaluated integrated flux transmissions for his radiation chart by taking $T = T_b$, yielding, for what will be called the Y technique,

$$\phi(\Sigma m, p, T; T_b) = \phi(\Sigma m_p, 1{\cdot}0, T = T_b; T_b) \quad . \quad . \quad (17)$$

Cowling (1950b) suggested that flux divergence be evaluated on the assumption that transmission depends only on the summation of absorbing mass, corrected for pressure and temperature, i.e.,

$$\phi = \phi\{\Sigma m(p, T)\}$$

This technique will be designated as CI. Assuming standard values of T and T_b where required, the CI technique can be symbolized by

$$\phi(m_p, 1{\cdot}0, T; 260) = \phi\{m(p, T), 1{\cdot}0, 260; 260\},$$
$$\phi(\Sigma m, p, T; T_b) = \phi\{\Sigma m(p, T), 1{\cdot}0, 260; 260\} \quad . \quad . \quad (18)$$

While the above stringent assumptions greatly simplify analytical calculations, a simple graphical evaluation is still possible if the transmission is permitted to depend on T_b, the

temperature of the black-body radiator involved. 260°K was still used as the standard temperature for the correction of absorbing masses of individual layers, but the actual temperature was used for T_b in obtaining $m(p, T)$, and the true radiating temperature was maintained for the net layer transmission. This technique, designated as CII, can be expressed mathematically as

$$\phi(m_p, 1{\cdot}0, T; T_b = T) = \phi\{m(p, T), 1{\cdot}0, 260; T_b = T\},$$
$$\phi(\Sigma m, p, T; T_b) = \phi\{\Sigma m(p, T), 1{\cdot}0, 260; T_b\} \quad . \quad . \quad (19)$$

Finally, three techniques arising from the results of the previous section were tested. In the first of these, GI, ϕ : (13) was used. In the second of these, GII, ϕ : (14) was used. In the third of these, GIII, the ϕ : (14) technique was employed but it was assumed that m_p represented the absorbing mass fully corrected for pressure, and only integrated flux transmissions valid at a pressure of one atm were employed.

The first set of numerical calculations utilized the computations of Table 4. Three of the four possible two-layer assemblies were studied : A and E, case I; B and F, case II; and D and H, case III. The values of m_p for each layer can be found in Table 4; case I embraces the thickest layers and case III the thinnest. As before, $p_1 = 1{\cdot}0$ atm, $T_1 = 300°$K, $p_2 = 0{\cdot}5$ atm and $T_2 = 220°$K. For each of the eight techniques outlined above, the radiative fluxes through the bottom of the multiple layer ($F\downarrow$) and through the top ($F\uparrow$) were evaluated, and are presented in Table 5, in ly/min (cal cm^{-2} min^{-1}). They may be considered as emitted fluxes from a simply-stratified atmosphere.

TABLE 5. FLUX EMISSION (LY/MIN) FROM A TWO-LAYER ATMOSPHERE

	Case : I		II		III	
Technique	$F\downarrow$	$F\uparrow$	$F\downarrow$	$F\uparrow$	$F\downarrow$	$F\uparrow$
E	0·4771	0·2567	0·2804	0·1415	0·1676	0·1132
R	0·5007	0·2390	0·3033	0·1296	0·1843	0·1099
Y	0·4906	0·2535	0·2936	0·1385	0·1785	0·1131
CI	0·5117	0·2493	0·3132	0·1399	0·1957	0·1245
CII	0·4884	0·2678	0·2896	0·1512	0·1777	0·1252
GI	0·4888	0·2677	0·2902	0·1516	0·1777	0·1219
GII	0·4885	0·2668	0·2900	0·1509	0·1777	0·1219
GIII	0·4885	0·2670	0·2899	0·1513	0·1776	0·1241

In Table 5, the GI technique may be considered to be highly accurate (relative to the spectral model and parameters adopted). On this basis, the first four techniques cannot be judged acceptable, since they produce errors exceeding five per cent for the upward-emitted flux. Except for the Yamamoto (Y) technique, the downward-emitted fluxes are also several per cent in error. Except for the upward-emitted flux in case III (very thin layers), the fluxes given by the last four techniques agree within less than 0·5 per cent. It may be concluded that both CII and GIII techniques adequately account for the variation of temperature, but neither are fully adequate for the pressure effect for very thin layers.

As mentioned earlier, it would be possible with a graphical technique to consider an appropriately-weighted mean pressure for the layer between the emitting layer and the reference level. This suggests a variation of the CII technique, known as CIII, which employs

$$\phi(m_p, 1{\cdot}0, T; T_b = T) = \phi\{m(p, T), 1{\cdot}0, 260; T_b = T\},$$
$$\phi(\Sigma m, p, T; T_b) = \phi\{\Sigma m(p, T), \bar{p}, 260; T_b)\} \quad . \quad . \quad (20)$$

To test the above technique adequately, a slightly more realistic model atmosphere than those of Table 5 is required. The following was assumed : at level 1, a black-body at 300°K; from level 1 to level 2, $m_p = 1{\cdot}0$ g/cm^2, $p = 1{\cdot}0$ atm, $T = 300$°K; from level 2 to level 3, $m_p = 0{\cdot}2$ g/cm^2, $p = 0{\cdot}5$ atm, $T = 260$°K; and from level 3 to level 4, $m_p = 0{\cdot}01$ g/cm^2, $p \simeq 0{\cdot}0$ atm, $T = 220$°K. If m_p is assigned a fixed value, it is possible to evaluate the limiting value of the integrated flux transmission as the pressure approaches zero. For example, the intensity transmission over a narrow spectral interval becomes Elsasser's error-function relation (Elsasser 1942).

For a number of computational techniques, the net upward radiative fluxes through levels 1, 2, 3, and 4 were evaluated, as well as the net flux increment for each layer (proportional to the mean cooling rate). The CI technique was not tested, and it was assumed that the GI and GII techniques would give virtually identical results for this model atmosphere. The resulting upward fluxes, in ly/min, are given in Table 6, for the E, R, Y, and GII techniques.

TABLE 6. NET UPWARD FLUXES, IN LY/MIN, FOR A THREE-LAYER ATMOSPHERE

Technique :	E	R	Y	GII
1	0·1913	0·1696	0·1786	0·1801
2	0·4784	0·4783	0·4783	0·4782
3	0·4831	0·4701	0·4738	0·4852
4	0·5069	0·4715	0·4898	0·5077
2 – 1	0·2871	0·3087	0·2997	0·2981
3 – 2	0·0047	– 0·0082	– 0·0045	0·0070
4 – 3	0·0238	0·0014	0·0160	0·0225

For the flux at level 1 and for the flux divergence from 1 to 2, the Yamamoto technique (Y) is clearly adequate, while the other two techniques are not. For the flux and flux divergence at upper levels (2 to 4), the Elsasser technique (E) is reasonably accurate, while the other two techniques are seriously in error. These conclusions may be due, in part, to the specific manner in which the principles were applied, and may be influenced by the crudity of the three-layer atmospheric model. However, the computations of Tables 5 and 6 suggest strongly that classical techniques of radiative computation (E, R, and Y) are not sufficiently accurate, largely as a result of the complex effects of temperature on emission and transmission.

In Table 7 are presented the net upward fluxes, in ly/min, for the CII, CIII, GII and GIII techniques. It may be concluded from Table 7 that the CIII technique is sufficiently accurate for practically every purpose.

TABLE 7. NET UPWARD FLUXES, IN LY/MIN, FOR A THREE-LAYER ATMOSPHERE

Technique :	CII	CIII	GII	GIII
1	0·1798	0·1798	0·1801	0·1802
2	0·4787	0·4784	0·4782	0·4786
3	0·4871	0·4852	0·4852	0·4871
4	0·5103	0·5087	0·5077	0·5094
2 – 1	0·2989	0·2986	0·2981	0·2984
3 – 2	0·0084	0·0068	0·0070	0·0085
4 – 3	0·0232	0·0235	0·0225	0·0223

If ϕ is evaluated from (20), it can simply be shown, by an extension of the developments of Elsasser (1942) and Yamamoto (1952), that the total radiative flux, as defined by (5), can be expressed as the following cyclic integral on a radiation chart :

$$F = \oint y \, dx,$$

where

$$x = F_b = \sigma T_b^4,$$

$$y = \phi + \frac{T_b}{4} \frac{\partial \phi}{\partial T_b},$$

the path of integration on the chart taking its usual form (Elsasser 1942). Here y is a function, from (20), of $\Sigma m\,(p, T)$, T_b, and $\bar{p}$, but its variation with $\bar{p}$ will be very small, except for very thin layers of adjusted absorbing mass. The basic isopleths on the diagram will be isotherms (vertical T_b lines) and lines of constant adjusted absorbing mass, $\Sigma m\,(p, T)$, the latter assuming $\bar{p} = p_0$. The vertical displacement for a value of $\bar{p}$ differing from p_0 can be indicated by additional sets of lines for $\bar{p} = \frac{1}{2} p_0$ and $\bar{p} = 0$, or by short segments of vertical lines giving directly $\{y\,(p_0) - y\,(\bar{p})\}$.

It will be necessary to devise a suitable technique for incorporating the effects of carbon dioxide, possibly along the lines suggested by Yamamoto (1952), and to obtain accurate values of the integrated flux transmission. It may be possible to use theoretical computations to correct thin-layer absorbing masses to a standard temperature, and then to use actual radiation measurements to obtain ϕ. Once again it might be necessary to make use of theory to obtain the variation of ϕ with T_b.

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The growth of raindrops in cloud

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Summary

A method is described by which the rate of growth of raindrops in cloud is deduced from the results of a balloon-borne radio transmitter. The transmitter, during its passage up through the cloud, gives a continuous record of the drop-size distribution of the rain. The values of the rates of growth of raindrops obtained in this manner differ considerably from those computed from accepted values of cloud-drop distributions and raindrop-collection efficiencies. The experimental values rise more rapidly than the computed values by a factor of from 3 to 4 in the 0·5 mm to 1·0 mm raindrop-diameter range.

It is concluded that the effect is real but, as yet, knowledge of the physical conditions within a cloud is insufficient to suggest a mechanism to account for it.

1. Introduction

The rate of growth of raindrops in an atmosphere of small cloud droplets is an important factor in most theories of rain formation, particularly in the case of rain from clouds which do not extend in height above the freezing level and which presumably consist of water in a liquid or gaseous condition only.

Attempts have been made to approach the subject by the determination of collection efficiencies, the collection efficiency of a drop being defined as the proportion of cloud droplets in the path of the drop which is collected by and becomes part of the drop, the drop being assumed spherical.

An indirect determination of collection efficiencies has been made by Langmuir (1948), and Gunn and Hitshfeld (1951) claim to have confirmed the figures in laboratory experiments over a very restricted range of raindrop and cloud particle diameters.

The following is a description of a method used at the C.S.I.R.O. Radiophysics Laboratory by means of which a more direct determination of raindrop growth in cloud under natural conditions was made.

2. Method

The observations are made by means of a balloon-borne radio transmitter operating at 60 Mc/s. The transmitter is frequency-modulated by a condenser-type microphone, consisting of a fixed metal plate and a metallized-rubber diaphragm upon which the raindrops impinge. The amount of modulation is therefore dependent upon the size of the raindrop.

As the microphone ascends through the rain a continuous sampling of raindrop size is transmitted. A ground-station receiver converts the signals to voltage pulses, whose magnitudes are dependent upon the sizes of the raindrops hitting the rubber diaphragm. These pulses are automatically sorted into pre-determined diameter ranges and the number of drops in each range is continuously recorded.

Each transmitter is also fitted with a baroswitch which cuts off the signal for a few seconds every 2,500 ft. From this information the rate of rise of the balloon may be determined. It is to be noted that in all cases there was no discontinuity in the upward velocity of the balloon.

A full description of the apparatus has been given by Cooper (1951).

3. Reduction of results

Each transmitter is calibrated before release by means of a water dispenser which gives a drop-diameter range of from 0·4 mm to 3·5 mm. Because the calibration of the transmitter is made while it is at rest, and the upward velocity of the balloon effectively increases the terminal velocities of the raindrops falling on the microphone, corrections have to be applied to convert calibration diameters to flight diameters. These corrections are discussed in Appendix A.

An unavoidable defect of the rubber-diaphragm microphone is that the sensitivity decreases from the centre to the edge, which means that the indicated diameter of a drop depends not only on its actual diameter, but also on the position at which the drop hits the microphone. Fortunately the indicated numbers in each size interval can be corrected to give the true number and these corrections are derived in Appendix B.

For reasons which will appear later it is convenient to present the results in the form of number of drops N of diameter greater than D, passing unit area in unit time, at various heights up through the rain, and this is derived from the actual counts in the following way.

Let H = height passed through by balloon

v_B = upward velocity of balloon

v_D = mean terminal velocity of drops in interval $D_1 \rightarrow D_2$

N_C = number of drops in interval $D_1 \rightarrow D_2$ counted during rise of balloon through H

N_V = number of drops in interval $D_1 \rightarrow D_2$ per unit volume of air

N_A = number of drops in interval $D_1 \rightarrow D_2$ passing unit area in unit time

A = area of microphone diaphragm.

Then the air volume of the column of drops in the range $D_1 \rightarrow D_2$ collected by the diaphragm equals $AH(v_B + v_D)/v_B$, therefore

$$N_C = N_V AH(v_B + v_D)/v_B$$

But

$$\begin{aligned} N_A &= N_V v_D \\ &= v_B v_D N_C/AH(v_B + v_D) \end{aligned}$$

Six flights were made under conditions of steady rain from thick stratus cloud with a lower layer of overcast. Tables 1-6 give the values of $N \times 10^4$ observed in these flights, as a function of height and drop diameter.

From these results the rate of growth of raindrops in cloud may be deduced, granted the following assumptions, which imply the existence of a steady state within the cloud, i.e., the size distribution of the rain at any particular height is independent of time.

(a) The number of raindrops of any particular size range entering the cloud remains unchanged during the passage time of the balloon through the cloud.

(b) During the same time all drops of diameter D grow to the same larger diameter D'.

(c) Within the small size range of raindrops measured, there is no appreciable interaction between the raindrops themselves.

The *a priori* justification of these assumptions is difficult if not impossible, the only observational point in support being that within each size range there is a simultaneous increase in the number of drops with a maximum at cloud base. However, the consistency of the results obtained by making these assumptions is in itself a justification, since it is unlikely that consistent results would be obtained from unsteady states on six different occasions.

TABLE 1. $N \times 10^4$

14 June 1949. Freezing level 8,000 ft. Overcast base 1,100 ft.

Height (1,000 ft)	Diameter D (mm) 0·45	0·70	0·90	1·10
0·25	1,702	1,121	156	44
0·75	1,751	564	51	
1·25	1,906	416	61	
1·75	1,075	93		
2·25	821	19		
2·75	319			
3·25	123			
3·75	463	365	122	
4·25	1,231	863	255	
4·75	962	471	51	

TABLE 2. $N \times 10^4$

18 March 1950. Freezing level 13,000 ft. Stratus base 9,000-10,000 ft.

Height (1,000 ft)	Diameter D (mm) 0·45	0·70	1·02	1·35	1·55
0	718	454	270	32	13
1	1,805	1,105	695	57	7
2	1,104	667	442	19	
3	654	209	117	22	13
4	880	221	170	51	7
5	579	258	130	76	20
6	474	128	77		
7	1,040	253	48		
8	1,398	281	76	16	7
9	1,485	364	149		
10	1,478	378	122	9	3
11	1,068	178	24	6	
12	625	15			
13	17	5			

TABLE 3. $N \times 10^4$

18 March 1950. Freezing level 13,000 ft. Stratus base 9,000-10,000 ft.

Height (1,000 ft)	Diameter D (mm) 0·46	0·70	0·91	1·11	1·33
2	376	75	43	13	
3	272	58	47	6	
4	182	55	28	13	7
5	245	49	12		
6	438	69	37	13	
7	507	97	86	9	3
8	566	101	74	27	14
9	752	229	184	78	21
10	639	166	91	32	7
11	428	105	30		
12	450	45			
13	435	21			

TABLE 4. $N \times 10^4$

3 April 1950. Freezing level 13,000 ft. Stratus base 9,000 ft.

Height (1,000 ft)	Diameter D (mm) 0·46	0·64	0·91	1·24
1	292	108	32	7
2	255	138	19	
3	391	148	19	
4	350	166	31	
5	551	213	100	
6	564	294	57	7
7	642	345	81	
8	934	274	42	7
9	1,219	486	92	7
10	1,132	412	56	
11	651	43		

TABLE 5. $N \times 10^4$

16 November 1950. Freezing level 10,000 ft. Stratus base 6,000 ft. Overcast base 2,000 ft.

Height (1,000 ft)	Diameter D (mm) 0·38	0·55	0·77	1·08	1·52
1	1,612	919	585	70	7
2	1,764	1,087	683	60	
3	1,726	1,027	594	73	
4	1,445	954	560	99	7
5	1,883	1,091	660	151	24
6	1,901	1,034	603	68	14
7	1,048	504	102	3	
8	802	151	11		
9	453	31			

TABLE 6. $N \times 10^4$

21 November 1950. Freezing level 10,000 ft. Stratus base 8,000 ft. Overcast base 1,000 ft.

Height (1,000 ft)	Diameter D (mm) 0·33	0·51	0·69	1·09
1	1,470	768	233	
2	1,424	1,090	355	
3	1,382	859	320	
4	1,604	991	461	34
5	1,269	760	337	17
6	1,413	742	322	28
7	1,502	741	284	
8	1,252	571	173	6
9	1,001	426	174	6
10	639	157	20	
11	524	84	15	

The steady state mentioned above may be pictured as follows. Consider a population of drops of one particular diameter, say D_1, at a particular height H_1. The drops, as they fall, gradually grow until at H_2 they have reached diameter D_2. The process is continuous, which means that there is a column of drops stretching from H_1 to H_2 varying continuously from D_1 to D_2, with the number of drops of diameter D_1 passing unit area at H_1 equal to the number of drops of diameter D_2 passing unit area at H_2. The whole rain column is made up of the sum of similar columns pictured above, and since the drops with diameters greater than D_1 at H_1 will grow to drops greater than D_2 at H_2, the number of drops greater than D_1 passing unit area at H_1 will equal the number of drops greater than D_2 passing unit area at H_2.

If the number of drops N greater than D which pass unit area in unit time is plotted against D, for various heights through the cloud, lines of equal N will cut the curves at points corresponding to the diameter of a growing drop at those heights. The curves from the results of the flight on 16 November 1950 for 8,000 ft, 7,000 ft and 6,000 ft are shown in Fig. 1.

From this, sets of initial and final diameters for each 1,000-ft fall through the cloud are obtained, which allow values of dD/dH to be computed. Fig. 2 is a plot of dD/dH against D obtained in this manner from the results in Tables 1 to 6.

An attempt to measure the evaporation of raindrops using the same technique did not give consistent results, probably because the effect being measured is small compared with the statistical errors inherent in the method.

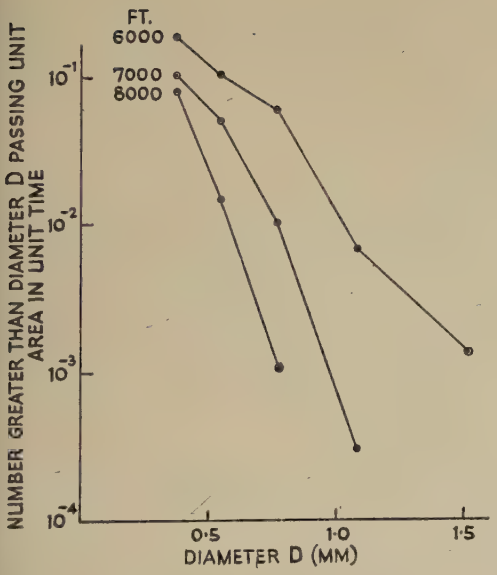

Figure 1. Sample-size distribution curves of rain at different heights within a cloud.

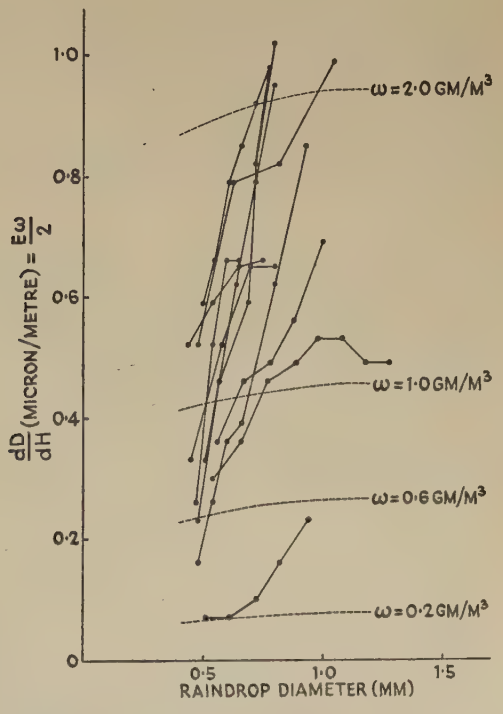

Figure 2. Growth of raindrops on falling through cloud.
•——• Experimental results.
– – – – Values computed from cloud size-distribution of Best (1951) and collection efficiencies of Langmuir (1948).

4. Discussion of results

Let ΔM = the increase of mass of a drop falling through a distance ΔH

D = drop diameter

ω = cloud-water content

E = collection efficiency of the drop integrated over the cloud spectrum

$$\Delta M = \frac{\pi D^2}{4} \omega E \, \Delta H$$

or
$$\frac{dM}{dH} = \frac{\pi D^2}{4} \omega E,$$

or
$$\frac{dD}{dH} = \frac{\omega E}{2}.$$

Using Langmuir's collection efficiencies and the cloud-size distribution given by Best (1951), values of dD/dH were computed for cloud-water contents from 0·2 to 2 g/m^3. These values are also plotted in Fig. 2. It will be seen that the values of $E\omega$ as measured for drops of diameter 0·5 mm are too small by a factor of about 4 or alternatively the measured values for 1 mm diameter drops are too large by the same factor.

Unfortunately no measurements of the water contents of the clouds could be made and it is thus impossible to separate the factors E and ω. Effective ω could conceivably be larger for the larger drops since they would be falling through both cloud droplets and the smaller raindrops, but that this can have little effect on the result is seen when the typical rain-water content below 0·5 mm is compared with cloud-water contents. According to Best (1950) for rain of 5 mm/hr there is only 0·006 g/m^3 contained in raindrops below 0·5 mm diameter which is negligible compared with cloud-water contents of at least 0·5 g/m^3.

Since then ω is effectively the same for both large and small drops, the values of E obtained from the above measurements vary considerably from those given by Langmuir (1948).

Another effect of interference between the larger and smaller raindrops is a reduction in the number of smaller drops. The following considerations however show that this effect is negligible.

Let $N = f(D)$ be the number of drops greater than diameter D passing unit area in unit time. The time taken for a small parcel of drops of diameter D_1 to fall H cm is given by H/v_1, where v_1 = terminal velocity of drop D_1. During this time a column of drops of diameter D of length $\left(\frac{v - v_1}{v_1}\right) H$ cm will pass through 1 cm^3 air volume of drops D_1. If now ΔN = number of drops in range $D \to D + \Delta D$ passing 1 cm^2/sec,

ΔN_c = number of drops per cm^3 of air in the same size range,

$$\Delta N_c = \frac{\Delta N}{v}.$$

In falling through 1 cm^3 air volume of D_1 drops, the drops in the range $D \to D + \Delta D$ will pick up

$$\frac{N_{c1} (v - v_1) H\pi (D + D_1)^2 \Delta N}{4 v_1 v}$$

of the D_1 drops (this assumes that contact between drops ensures coalescence). Therefore the total number of drops of diameter D_1 per cm^3 of air, picked up by drops of diameter greater than D_1,

$$= \frac{H\pi N_{c1}}{4v_1} \int_{D_1}^{D_{\max}} \frac{v - v_1}{v} (D + D_1)^2 f'(D)\, dD$$

or, putting $v = kD$, the proportion of D_1 drops collected by all drops greater than D_1

$$= \frac{H\pi}{4v_1} \int_{D_1}^{D_{\max}} \frac{(kD - v_1)}{kD} (D + D_1)^2 f'(D)\, dD$$

This integral reduces to the sum of three integrals

$$\frac{H\pi}{4v_1} \left[D_1^3 \int_{D_1}^{D_{\max}} f(D)\, d\left(\frac{1}{D}\right) - D_1 \int_{D_1}^{D_{\max}} f(D)\, dD - \int_{D_1}^{D_{\max}} f(D)\, d(D^2) \right]$$

which may be determined graphically from the flight results. Using the results at the 9,000-ft level from the flight on 3 April 1950 the sum of the above integrals, with $D_1 = 0{\cdot}5$ mm and $H = 1{,}000$ ft, is 0·036. This proportion of 0·5 mm drops picked up by larger drops does not significantly affect the assumption of no interaction.

A possible interpretation of the above results is that Langmuir's assumption that coalescence of a raindrop and a cloud droplet always occurs whenever they come into contact is not necessarily valid for all drop sizes, but that the proportion of occasions when coalescence does not occur decreases rapidly with increase in diameter. This view however must assume that the water contents of the clouds through which the rain passed were mainly greater than 2 g/m^3 and that for raindrops of 0·5 mm up to 75 per cent of the cloud droplets coming into contact with the drop do not coalesce with the drop. Best (1951) in a survey of cloud measurements quotes the highest cloud-water content in stratocumulus clouds measured by Frith (1951) as 0·83 g/m^3.

The alternative to reduced collection efficiencies and high water contents is cloud-water contents up to 1 g/m^3 and a rapid increase of collection efficiencies from 1 at 0·5 mm drops to 4 at 1 mm drops. It is difficult to see at this stage what physical process or processes could produce such rapid increases in and large values of collection efficiencies, but the possibility of its occurrence cannot be definitely ruled out until such time as more detailed information is available on the physical conditions applying both to raindrops and cloud droplets in the natural state.

5. Conclusions

The rate of increase with drop diameter of the collection efficiencies of raindrops in a cloud atmosphere has been found to be greater by a factor of 3 to 4 than the rate obtained from currently accepted figures. A careful investigation of the method of observation has failed to suggest how the method could introduce a consistent error of such a magnitude and the conclusion is forced that the effect is real. There is as yet insufficient information available to suggest a mechanism which could account for such an effect.

6. Acknowledgments

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Appendix A

The conversion of calibration diameters to flight diameters would appear at first sight to be a matter of determining equivalent diameters on an equal momentum basis.

Drops of diameters ranging from 2·1 mm to 3·6 mm were dropped from heights varying from 1·5 ft to 4 ft. Using values of velocities given by Laws (1941) for water drops after varying distances of fall, the momenta of the drops were calculated and plotted against receiver volts as shown in Figs. 3, 4 and 5. It will be seen that equal momenta do not give equal receiver volts. However by plotting the kinetic energies of the drops against receiver volts as shown in Fig. 4 it is clear that equal kinetic energies do give equal receiver volts and the conversion from calibration to flight diameters was done on this basis. Fig. 5 gives the conversion for two different rates of rise of the balloon.

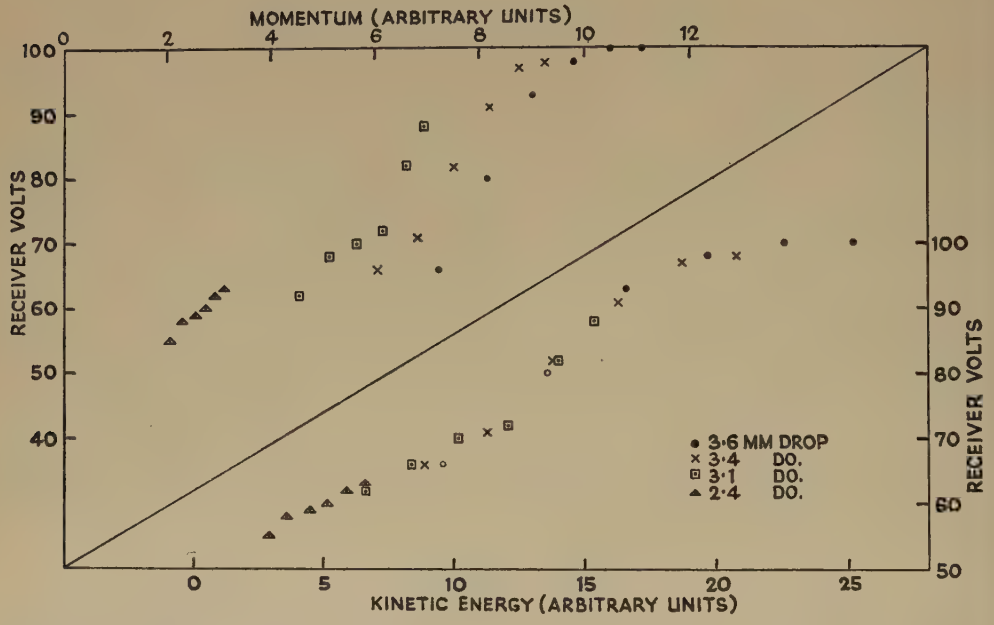

Figure 3 (upper half of diagram). Momentum versus receiver volts for four different drops falling at different velocities.

Figure 4 (lower half of diagram). Kinetic energy versus receiver volts for four different drops falling at different velocities.

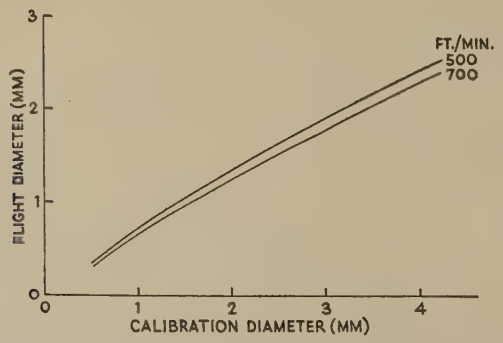

Figure 5. Calibration diameters versus flight diameters for two different rates of rise of the transmitter.

Appendix B

An inherent defect of the raindrop microphone is that the sensitivity of the diaphragm decreases from the centre to the edge. A drop of diameter D falling at the edge of the diaphragm gives an impulse equal to that of a drop of diameter xD falling at the centre where $x < 1$. A large amount of this variability has been removed in the present microphone by making the full diaphragm 3-in. diameter and masking off all but a 2-in. diameter in the centre with a ring of felt which also acts as a damping agent and reduces the time resolution of the microphone.

Over this 2-in. circle the decrease in sensitivity from the centre to the edge of the diaphragm was found to be linear with distance from the centre with x varying from 0·65 to 0·80 from diaphragm to diaphragm so that if

r = the distance from the centre of the diaphragm at which a drop of diameter D falls,

R = the radius of the diaphragm.

The indicated diameter D' of the drop is given by

$$D' = D\left[1 - (1 - x)\frac{r}{R}\right]$$

or

$$r = \frac{R}{(1 - x)}\left(1 - \frac{D'}{D}\right)$$

gives the distance r from the centre of the diaphragm at which a drop of diameter D must fall to be indicated as a drop of diameter D'.

Suppose now that the drops are divided into class intervals $D_n \to D_{n-1}$, $D_{n-1} \to D_{n-2}$, etc. ($D_n > D_{n-1} > D_{n-2} \ldots$) and that $D_n > D > D_{n-1}$. Then all those drops of diameter D which fall between $r_1 = \frac{R}{(1 - x)}\left(1 - \frac{D_{n-1}}{D}\right)$ and $r_2 = \frac{R}{(1 - x)}\left(1 - \frac{D_{n-2}}{D}\right)$ will be indicated as falling into the class interval, $D_{n-1} \to D_{n-2}$.

If the class intervals are chosen so that

$$D_{n-1} = xD_n,\ D_{n-2} = xD_{n-1},\ \text{etc.}$$

r_2 becomes R for $D = D_{n-1}$ the smallest drop in the interval $D_n \to D_{n-1}$ and the proportion of drops of diameter D ($D_n > D > D_{n-1}$) which will be indicated as falling into the next interval below is given by

$$P = \frac{R^2 - r_1^2}{R^2}.$$

If we assume a uniform horizontal distribution of the drops of diameter D

$$P = 1 - \frac{r_1^2}{R^2}$$

$$= 1 - \frac{1}{(1 - x)^2}\left(1 - \frac{D_{n-1}}{D}\right)^2.$$

Let N be the number of drops in the interval $D_n \to D_{n-1}$ and $n = \frac{N}{D_n - D_{n-1}} = \frac{N}{(1 - x)D_n}$ be the number of drops per unit-size range in the same interval.

Of the drops in the size range, $D \to D + \Delta D$ [$D_n > D > D_{n-1}$], $Pn\,\Delta D$ drops will be indicated in the range $D_{n-1} \to D_{n-2}$. The *total* number of drops in the interval $D_n \to D_{n-1}$ indicated in the range $D_{n-1} \to D_{n-2}$ will therefore be N_1 where

$$N_1 = \int_{xD_n}^{D_n} n \left[1 - \frac{\left(1 - x\frac{D_n}{D}\right)^2}{(1-x)^2}\right] dD$$

$$= \frac{N}{(1-x)^2}\left[x^2 - 3x - \frac{2x \log x}{1-x}\right]$$

or if P is the proportion of drops in one interval indicated as falling into the next interval below

$$P = \frac{N_1}{N}$$

$$= \frac{1}{(1-x)^2}\left[x^2 - 3x - \frac{2x \log x}{1-x}\right]$$

Fig. 6 is a plot of P against x.

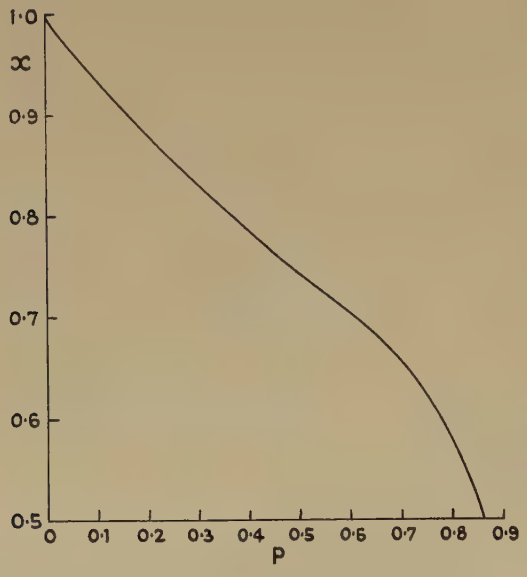

Figure 6. Plot of P, the proportion of drops in any particular size range indicated in the size range below, versus x the ratio of indicated diameter of a drop falling at the edge and at the centre of the diaphragm.

Given now that the actual record has n_1, n_2, n_3 etc., drops in the given intervals starting with n_1 in the interval of greatest mean diameter we may deduce the true number of drops in each interval in the following manner.

Since there were no bigger drops recorded than those included in n_1 and since these represent only $(1 - P)$ of the drops actually belonging to this interval the true number of drops N_1 in this interval is given by

$$N_1 = \frac{n_1}{1 - P}.$$

The next interval below with n_2 recorded drops will contain PN_1 drops from the higher interval so the true number of drops N_2 in this interval is given by

$$N_2 = \frac{n_2 - PN_1}{1 - P}$$

Similarly

$$N_3 = \frac{n_3 - PN}{(1 - P)^2}$$

551.577.2 (62) : 551.577.3 : 551.515.8

Rainfall over Egypt

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(Manuscript received 6 December 1952, in revised form 6 March 1953)

Summary

The problem of rainfall over Egypt has been investigated.

The distribution of annual amounts of rainfall is given and remarkable variations along the Mediterranean coast are discussed.

Winter is the season of rain in the north of Egypt, but in the south (Upper Egypt), two main seasons are found, namely, May and October.

The diurnal variation of rain of two coastal stations and one inland station is given and discussed.

Warm-front rain practically does not occur, and cold-front rain forms only a small proportion of the total amount and occasions of rain. Most rainfall is found to be non-frontal and associated with upper *cold* troughs (or lows) which seem to play an important role, not only in Egypt, but all over the Eastern Mediterranean.

Rainy situations in each season are discussed. It is found that, in winter, rain is generally associated with Mediterranean depressions, but when these depressions are not associated with upper cold troughs, they move quickly with very little precipitation. In the spring and autumn, however, rain is usually associated with small desert lows with cold upper lows or troughs.

1. Introduction

Egypt and the eastern Mediterranean have become an important junction of main air routes of the three continents which meet there, and accurate weather forecasting over that junction is necessary.

Difficulty is, however, encountered in relating clouds and precipitation to fronts (Bjerknes scheme). Moreover, most of what has been written on the problem of rainfall over the area does not afford a solution, and upper-air charts have not been given proper consideration.

The investigation of thunderstorms and floods over Egypt (Soliman 1946) in the transitional seasons gave encouraging results for further thorough investigation of the problem of rainfall over Egypt and the eastern Mediterranean.

2. Distribution of rainfall

The distribution of the mean annual amounts of rain over Egypt (Fig. 1) shows a maximum on the Mediterranean coast with a rapid decrease inland. Egypt south of 28°N. is practically rainless. This is due to the fact that most of the rain is associated with Mediterranean depressions and upper troughs which affect mainly the coastal regions.

An outstanding feature of Fig. 1 is the uneven distribution of rainfall along the Mediterranean coast, the maximum amounts occurring on the western part of the Delta Coast (Alexandria to Borolos) where they average about 180 mm/year; they decrease, eastward, to about 80 mm in the area of Port Said. This results from the configuration of the coast line, since most of the coastal showers occur with a W. to NW. air current which would first meet the western part of the Delta coast and shed most of its rain before reaching the eastern coastal regions. It should be stated that the Delta is flat and that coastal showers are not orographic, but are due to the deviation from the geostrophic wind caused by surface friction at the coast line, which produces convergence in the unstable air currents, and to the increased instability by day over the heated land. It will also be seen later that this configuration of the coast line leads to some night showers over the west part of the Delta coast in connection with land breezes.

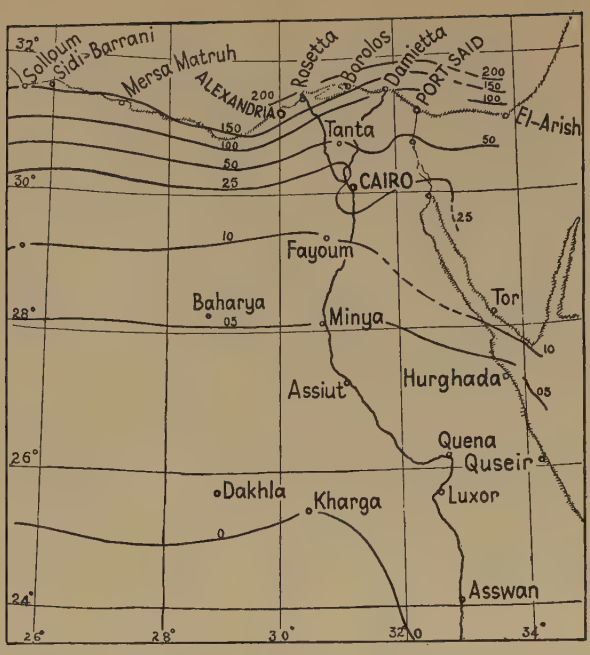


Figure 1. The distribution of mean annual amounts of rainfall (mm) over Egypt. Data up to 1945 are used.

Most of the rain in Egypt falls in the form of showers and the amounts of rainfall may, therefore, vary considerably from year to year in the same place, and may also differ in two neighbouring localities in the same year. Table 1 gives the maximum amounts of rainfall in one day as compared with the mean annual amounts. The data in Table 1 show frequent occurrence of the absolute maximum in one day during the transitional periods (especially in October and November). This is mainly due to heavy thundery showers associated with transitional small upper lows or troughs as will be shown later.

TABLE 1. The maximum amounts of rain in one day as compared with the annual means for different stations in Egypt (data up to 1945)

Station	Max. in one day (mm)	Date	Annual Mean (mm)	Station	Max. in one day (mm)	Date	Annual Mean (mm)
Solloum	46·0	17.11.35	95	Hurghada	41·0	8.11.39	3
Sidi Barani	76·0	27.10.13	149	Qusier	34·0	6.11.34	4
Mersa Matruh	98·8	28.12.30	158	Cairo (Ezb)	43·2	17. 1 19	26
Alexandria	62·4	5.11.38	184	Giza	53·2	27.10.37	24
Rosetta	49·0	27.10.37	173	Helwan	37·3	24.4.08	31
Damietta	54·2	12.1.12	102	Fayoum	44·0	30.12.44	17
Port Said	58·0	24.2.12	79	Siwa	28·0	28.12.30	10
El-Arish	52·0	12.10.40	97	Minya	13·2	29.1.38	2
Damanhour	54·5	26.1.08	99	Assiut	20·0	6.11.34	7
Tanta	39·0	27.10.37	49	Baharya	14·0	29. 2.40	5
						26.11.36	
Mansourah	48·0	27·10.37	55	Quena	11·7	31.12.44	4
Edfina	42·0	27.10.37	159	Luxor	4·0	12.10.40	1
Belquas	75·0	26.1.08	69	Dakhla	8·0	15.2.42	1
Suez (P.T.)	32·3	9.11.39	21	Asswan	5·5	10.5.35	3
Tor	37·4	8.11.25	13	Kharga	7·5	22.12.43	0

3. Seasonal variation of rainfall

Rain occurs in the cold season from October to May; but occasionally a shower may fall early in June, or late in September. Otherwise it can be safely said that summer is practically rainless.

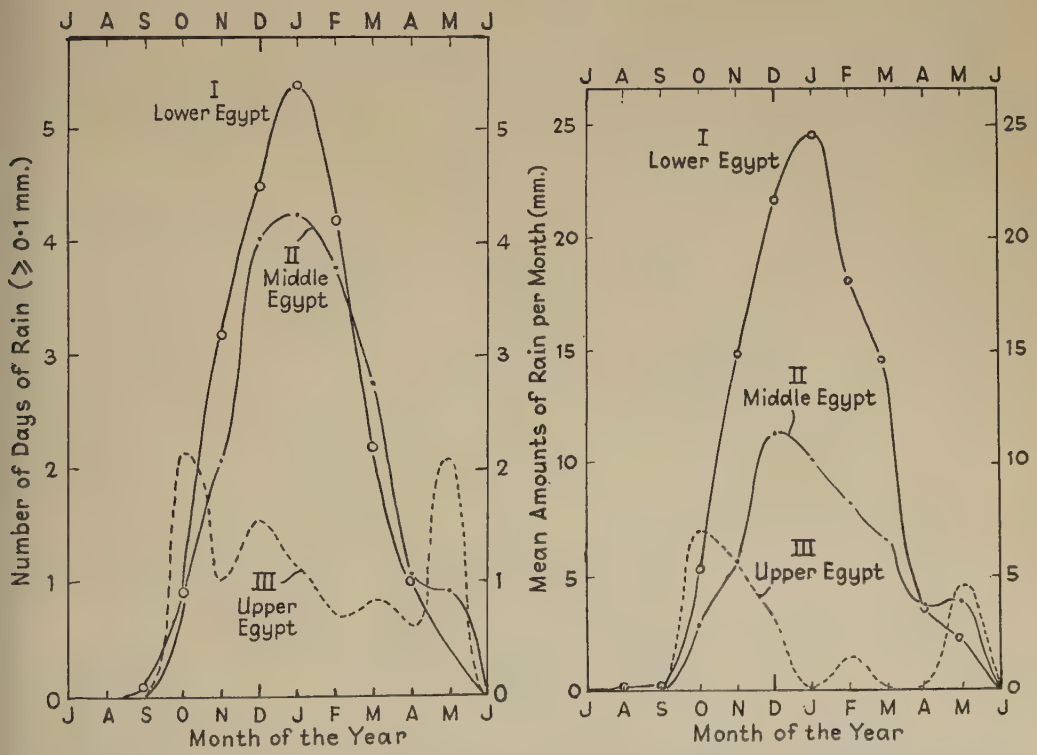
The rainiest months vary according to locality, and Egypt can be divided in this respect into the following regions :

Region I – Mediterranean coast and Lower Egypt (to 30°N)
Region II – Middle Egypt (30°N-27½°N)
Region III – Upper Egypt (27½°N-22½°N)

The seasonal variation of the number of days of rain (with at least 0·1 mm) for each of the three regions is given in Fig. 2 (a) which is based on means of 20 stations in region I, 9 stations in region II and 7 stations in region III. Data up to 1945 are used.

Fig. 2 (a) shows that region I has one maximum in winter, centred in January, from which the curve falls off smoothly on both sides. This conforms with the fact that most of the rain in that region is caused by winter Mediterranean depressions. On the other hand, the curve for region III has two marked maxima, one in October and the other in May (transitional months), and a third less marked winter maximum.

We see, therefore, that winter Mediterranean depressions are not the principal cause of rainfall over Upper Egypt. Investigation has shown that desert depressions and upper cold lows (or troughs) are responsible for these two maxima.



(a) Average number of days with at least 0·1 mm of rain. (b) Average amount of rainfall (mm) in each month.

Figure 2. Seasonal variation of rainfall for the 3 regions : Lower, Middle and Upper Egypt. Ordinates of curves II and III are enlarged 3 and 10 times respectively, in each case.

The curve for region II, Middle Egypt, shows that this region is affected by both causes, but is mainly affected by Mediterranean winter depressions, and thus has only one main maximum in January, but the effects in October and May still show up slightly, especially the latter. It is concluded that the transitions are the main seasons of rain in Upper Egypt and not winter as was thought to be the case.

Curves for the seasonal variation of the *amounts* of rainfall (Fig. 2 (b)) show similar results, but with the following remarks :

Region II. The peak of the curve lies in December when precipitation seems to be more intense than in January.

Region III. (a) The October maximum is more pronounced than that in May. This is due to the higher humidity of the air in autumn than in spring.

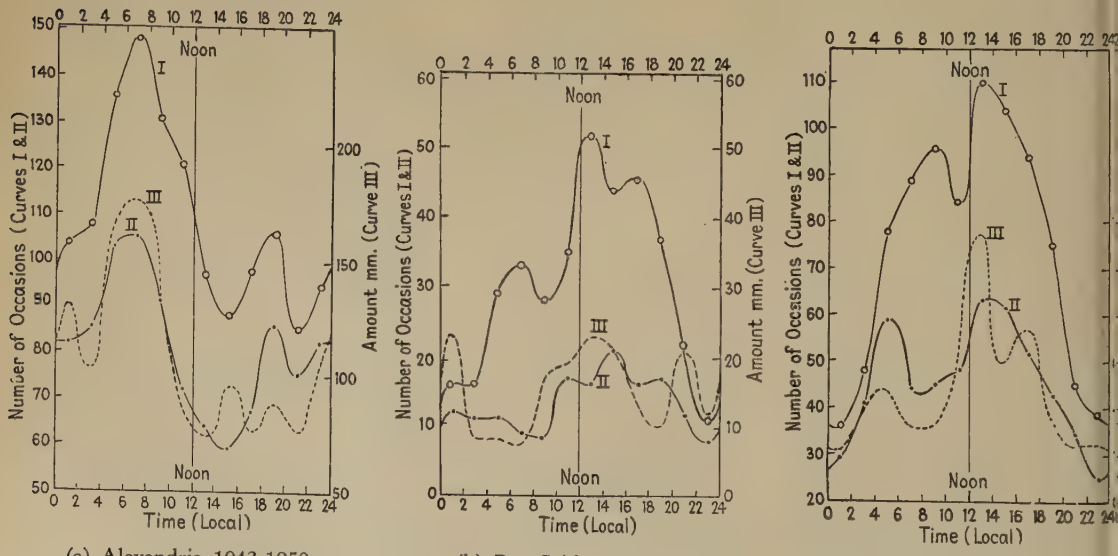
(b) The winter maximum is here less pronounced; even when this region is affected by winter depressions, the rain is slight, while in the transitions rain is usually heavier.

4. The diurnal variation of rainfall

There are two types of diurnal variation of precipitation, the maritime type, which has its maximum in the early morning or during the night, and the continental type, which has its maximum in the afternoon. This does not mean that every continental station will have its maximum in the afternoon or that every coastal station will be of the maritime type; also, a station may have a double precipitation maximum, one in the early morning and the other in the afternoon.

Goldie (1931) found that, for some stations in Great Britain, cold fronts and warm fronts have diurnal variation. The principal maximum occurred in the early morning in the case of warm fronts, and in the afternoon in the case of cold fronts, while in homogeneous polar air it was negligible.

The diurnal variation for some stations in Egypt was examined. Since rainfall is generally small and confined to Lower Egypt, and the study requires continuous observations by day and night, it was only possible to study a limited number of stations for the period between 1941 and 1950.



(a) Alexandria, 1943-1950. (b) Port Said, 1941-1950. (c) Cairo (Almaza), 1942-1948.

Figure 3. Diurnal variation of rainfall. Curve I – number of occasions with a 'trace' or more rainfall. Curve II – number of occasions of rainfall exceeding 0·1 mm in 2-hr period. Curve III – rainfall amounts in mm (scale on right).

Figs. 3 (a), (b) and (c) give the diurnal variation for the three cities, Alexandria (airport), Cairo (Almaza airport) and Port Said (airport) respectively, for the periods shown on the figures. In each figure three curves are given :

Curve I – For the frequency of occurrence of rain of any amount including trace.
Curve II – For the frequency of occurrence of rain of amounts $\geqslant 0{\cdot}1$ mm.
Curve III – For the amounts of rainfall in mm.

The frequency of occurrence is compiled in intervals of 2 hr. If rain occurs more than once in the same period (on the same day), it is only counted once.

Fig. 3 (a) shows that rain in Alexandria is mainly a morning phenomenon (maritime); the main maximum lies between 6-8 LT in the morning, with a secondary maximum in the evening.

Fig. 3 (b) for Cairo shows a general main maximum in the afternoon with the peak in the early afternoon, and a secondary maximum in the morning.

Fig. 3 (c) shows that for Port Said, despite its being a coastal station, the main maximum lies in the early afternoon (like Cairo), with a secondary maximum in the late morning; but the difference between the two maxima is small, a behaviour intermediate between those at Cairo and Alexandria. Curve III indicates, however, that the bulk of the rain in Port Said falls in the afternoon when the showers are more intense. Fig. 3 (c) also indicates that the late morning showers of the secondary maximum are usually very light.

(a) *The effect of land breeze on the diurnal variation of rain*

Rain falls in the cold season in Egypt when, at night, the air temperature over the land is much lower than that over the sea. The difference between the mean minimum temperature over the Nile Delta and the temperature of Mediterranean sea water, during the cold season, October-March, varies between 8°C and 12°C.

The wind records at Alexandria show that a land breeze frequently sets in suddenly in the very early hours of the morning, blowing from a southerly direction with a speed of about 10-15 km/hr. The average depth as judged from upper winds and tephigrams is about 1,000 ft. A favourable condition for a land breeze to occur is when a W.-NW. stream blows over the eastern Mediterranean with a weak pressure gradient.

Such a land breeze, besides acting as a minor cold front, affords a zone of convergence and leads to the development of clouds and precipitation. Such clouds will be carried by the upper winds (W.-NW.) and drift eastwards or south-eastwards. It is obvious that the share of the western part of the Delta coast of such rain will much exceed that of the eastern part, as a result of the configuration of the coast line in relation to the air current. This provides an explanation of the fact that the early morning maximum in Alexandria is more pronounced than that in Port Said.

5. Rain in relation to fronts

Rain in Egypt can be divided into two categories, frontal and non-frontal.

(a) *Frontal rain*

(i) *Warm-front rain.* This rain, produced by the ascent of the warm air over the cold air in advance of the depression, practically does not occur over Egypt for the simple reason that the air in the warm sector is very dry (Tc), originating from over the deserts of North Africa, Arabia and Egypt itself. The warm fronts of eastern Mediterranean depressions are therefore usually situated north of Egypt, so that warm-front rain, if it occurs, would fall further north. The synoptic charts of 28 Dec. 1950, and 31 Jan. 1950 afford examples.

(ii) *Cold-front rain.* Since the air in the warm sector is always dry, as mentioned above, its lifting on the cold front would also give no rain, except on the coast and over the Mediterranean waters where the warm air picks up some moisture. This is found in practice to be the case with most primary cold fronts.

The air behind the primary cold front usually comes from a W. or NW. direction and has had a fairly long track over the Mediterranean, thus becoming moist and modified.

When the depression reaches the east Mediterranean, especially the Cyprus area, it draws in its rear cold air from the Balkans (Pc), which will come to Egypt from a northerly direction and a short sea-track, behind a second cold front. It is this second type of cold front that is responsible for the cold-front rain which falls over Egypt, not only on the coast, but also inland.

It must be stated clearly that most of the rain which falls over Egypt is, however, non-frontal and that cold-front rain forms only a small proportion of the total amount and frequency of rain (see Table 2).

(iii) *Occlusion rain.* Rain from occluded fronts is uncommon in Egypt, and only occurs further north or west.

(b) *Non-frontal rain*

It has been stated before that most of the rain that occurs over Egypt and the eastern Mediterranean could not be explained by the Bjerknes frontal system.

The rain that fell over Cairo, Alexandria and Port Said during the period Dec. 1951 to Nov. 1952 inclusive, has been analysed and results are given in Table 2. In this analysis the records of wind, temperature, pressure and rain, together with surface and upper air charts, were carefully examined.

TABLE 2. Analysis of rainfall over 3 stations in lower Egypt during the period Dec. 1951-Nov. 1952

	Alexandria	Port Said	Cairo
Rain due to cold fronts (mm)	9·8	trace	trace
Rain due to instability in absence of upper troughs or lows (mm)	8·4	1·7	trace
Rain due to upper cold troughs or lows (mm)	173·2	105·2	69
Number of days of cold-front rain	3	3	2
Number of days of mere instability rain	6	5	3
Number of days of rain associated with upper cold troughs or lows	70	33	23

This shows that most of the rainfall is associated with cold troughs or lows in the middle troposphere, and that while warm-front rain does not exist, cold-front rain forms only a small proportion of total amount and frequency.

These facts are supported by long experience in day-to-day weather analysis.

6. Rainy situations in different seasons

As an aid to the forecasting of clouds and precipitation over Egypt, the important pressure situations that usually lead to widespread precipitation in each season are here given and discussed.

(a) *In winter*

The most important situation in this season is the presence and persistence of low pressure over the east Mediterranean, usually centred over Cyprus and called a ' Cyprus low.' This is known to give, sometimes, the worst type of weather over the Eastern Mediterranean. Investigation of several Cyprus lows showed interesting facts which can be summarized in the following :

(1). Upper air cold lows (or troughs) are again found to be a major and controlling factor in the intensity of precipitation, the area affected, and the persistence of the low over the area. The presence of a cold low on the 500 mb chart, is associated with heavy widespread rain and thunderstorms. The upper low usually remains stationary for 2-3 days during which frequent showers occur by day as well as by night. Such showers have been explained by many successive cold fronts, which is incorrect; they are produced by convergence and instability. Since the temperature gradient in upper cold lows or troughs over the eastern Mediterranean is from S. to N. (colder in the north), and since the sea-water affords a good supply of moisture to the air over it, the intensity of precipitation in general increases with latitude. Furthermore, the orography of Cyprus, Lebanon and Palestine enhances the frequency and violence of the convection, leading to severe thunderstorms.

(2). In the absence of upper lows or troughs, surface depressions move without giving any appreciable precipitation, and do not tend to become stationary, as Cyprus lows, on reaching the Eastern Mediterranean.

Precipitation associated with lows over the Eastern Mediterranean varies considerably from one case to another. Extensive lows with a steep pressure gradient, but with a flat or only slightly cold upper trough above, give little precipitation, while others which are smaller or shallower, but with cold upper troughs, may be associated with heavy and widespread precipitation. Examples of the first type are : 10 Mar. 1952; 1 Feb. 1950. Examples of second type are : 6-8 Jan. 1951; 6 Dec. 1951; 18-24 Dec. 1951; 22 Feb. 1952.

Figs. 4 (a) and 4 (b) show the example of 1 Feb. 1950. The surface chart indicates a deep ' Cyprus low '; but with a very flat and slightly cold upper trough. This low gave very little precipitation. For instance, rain was only reported by two coastal stations in Egypt that day, and by two others the following day. The total amount that fell at the four stations was 1·2 mm.

Figs. 5 (a) and 5 (b) give the conditions on 21 Dec. 1951 (the middle of the period 18-24). In this case the depression had a well defined *cold* low, aloft, and was associated with frequent heavy showers all over Lower and Middle Egypt during that period.

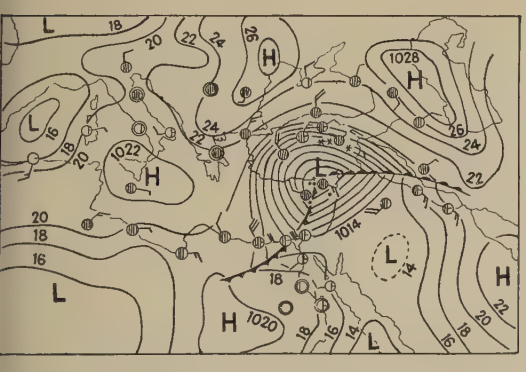


Figure 4 (a). Surface chart at 06 GMT on 1 Feb. 1950.

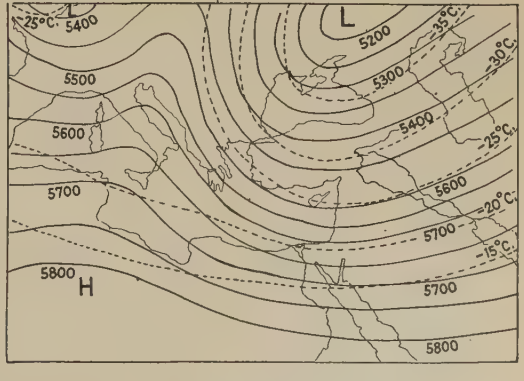


Figure 4 (b). 500-mb contour chart at 03 GMT on 1 Feb. 1950.

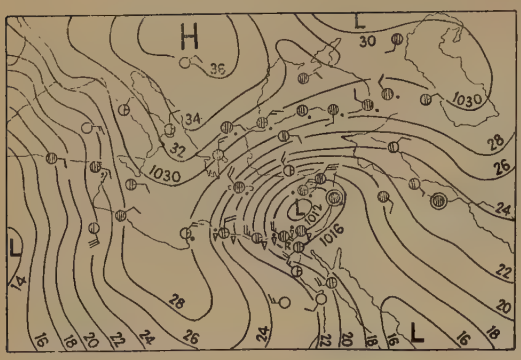


Figure 5 (a). Surface chart at 06 GMT on 21 Dec. 1951.

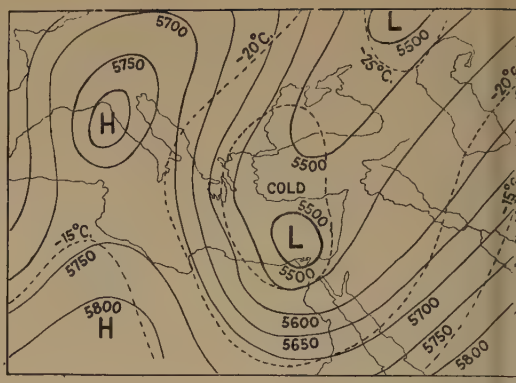


Figure 5 (b). 500-mb contour chart at 03 GMT on 21 Dec. 1951.

(b) *In spring*

The track of many Mediterranean depressions in spring is shifted southwards, and the centres pass either along the African coast or further south as desert depressions (Khamsin depressions).

When such lows are shallow (i.e., do not show in 700-mb and 500-mb charts) they give little or no precipitation over Egypt. If they show in upper-air charts (as a cold centre of the low or trough) then they are associated with much high and medium clouds, even if they pass as completely desert depressions, and give showers of rain, sometimes thundery in character. These lows are responsible for spring rain and, although their main tracks are from W. to E., yet they may move in any other direction, which depends on the track of the upper cold low (Soliman 1946).

It must be explained here that in the transitional seasons most of the rain, and high and medium clouds, are middle tropospheric phenomena (3-8 km). With the heating of surface layers by insolation over land, instability may reach from the surface to great heights, producing severe vertical currents, squalls and local sandstorms.

(c) *In autumn*

Rain in this season is due to similar conditions to those in spring, but the depressions are not so well marked; also they are usually smaller and with a feebler pressure-gradient.

Such rainy lows seem to be mere reflections of the upper air lows or troughs and their slow movement is also related to the upper low. Autumn lows may reach Egypt from the north, south, or even east, the W.-E. track being less predominant than in spring.

One of the important rainy situations in this season results from the northward extension of the Sudan low appearing as a trough (inverted V) over the north Red Sea area, and the subsequent development of thunderstorms. Rain and thunderstorms in this situation have been explained by some writers (Ashbel 1938) by the rapid lifting of the SE. current by colder northerly air from the Mediterranean. It has been shown, however (Soliman 1946), that such thunderstorms are due to upper cold lows or troughs leading to instability and convergence (enhanced in places by topography). In other words, the phenomenon is not frontal as suggested, since the same warm SE. current turns round the trough and blows over Egypt as NE. winds. It is also found that the mere northward extension of the Sudanese low without the cold upper lows or troughs fails to give more than some high and sometimes thin medium clouds.

Acknowledgments

I wish to express my indebtedness to Dr. A. M. Firesah for his advice and keen interest throughout the present work. I also wish to thank the staff of the Meteorological Department, Cairo, for supplying the data used in this investigation.

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551.551

The onset of turbulence in an elevated layer near sunrise*

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Summary

Abrupt changes at heights 46 m and 125 m above the ground from smooth flow, or flow in which damped eddies occur, to a flow characterized by continuing gustiness are investigated in terms of a critical Richardson number. Such transitions at Upton, N.Y. during the period October-November 1950 are identified from continuous records of the wind direction. Account is taken of the time for the transition to propagate across the layer. Fourteen transitions during the period studied, all near sunrise, indicate that the (layer) critical number for eddies with periods somewhat less than one minute lies within the range from $\frac{1}{3}$ to $\frac{1}{2}$. It is suggested that the method used may be applicable to other eddy sizes.

1. Introduction

The transition from laminar to turbulent flow continues to be one of the major interests of aerodynamics (Munk 1951). Strictly laminar flow is seldom observed in the atmosphere however, since the flow is usually characterized by large Reynolds numbers (Lettau 1939). Paeschke (1937) reported that the wind speed, as measured by the rapidly responding hot-wire anemometer, was always quite irregular near the ground even in very stable density stratifications. Nevertheless, if we fix our attention upon eddies of somewhat larger scale, such as are indicated by the oscillations of a wind vane or a pitot-tube anemometer, we find regimes of smooth and disturbed flow in the atmosphere near the ground which resemble those observed on a much smaller scale in the wind tunnel.

Giblett *et al.* (1932) classified atmospheric flow near the ground into several types, of which smooth flow and damped eddies (Giblett's type IV), and what we shall call turbulent flow (his types III and II) are of interest here. Fig. 1, in which continuous records of the wind direction at 46 m and 125 m above the ground are reproduced, illustrates smooth flow at a, damped eddies at b, and turbulent flow at c. These records are typical of many obtained at sunrise after a night with considerable heat loss from the ground and lower atmosphere when the wind above the friction layer is not too strong.

The transition from laminar to turbulent flow in the atmosphere, in the presence of vertical gradients of wind velocity and temperature, was analyzed by L. F. Richardson (1920, 1925). The Richardson number

$$Ri = \frac{g}{T}\left(\frac{\partial T}{\partial z} + \Gamma\right) \Big/ \left|\frac{\partial \overline{\mathbf{V}}}{\partial z}\right|^2$$

has since come into use as a parameter in atmospheric turbulence investigations. Here z is the height, T is the mean absolute temperature, $\overline{\mathbf{V}}$ is the mean horizontal wind velocity (vector), Γ is the adiabatic lapse rate, and g is the acceleration of gravity. Richardson concluded that turbulent energy will increase, starting from very small magnitudes, if the value of Ri was less than the ratio of eddy viscosity to eddy conductivity, which he, like Taylor (1915), assumed to be unity.

Richardson's work has been followed not only by new theoretical investigations, but also by experimental determinations of the critical Richardson number. Reference is made to summaries by Sutton (1949), Priestley and Sheppard (1952), and Goldstein (1938). The criterion for onset of turbulent flow appears to vary with height, and the results of theory and of measurements in the atmosphere are not in good agreement. Doubts have been expressed as to the

* This research was carried out at Brookhaven National Laboratory under the auspices of the U.S. Atomic Energy Commission. The present paper is based upon a thesis which the author submitted in January 1952, in partial fulfilment of the requirements for the degree of Master of Science at New York University. A preliminary report was given in a paper entitled, 'Onset of turbulence near the ground after sunrise and the Richardson criterion,' which the author read on 2 May 1951 at the 110th national meeting of the American Meteorological Society in Washington, D.C.

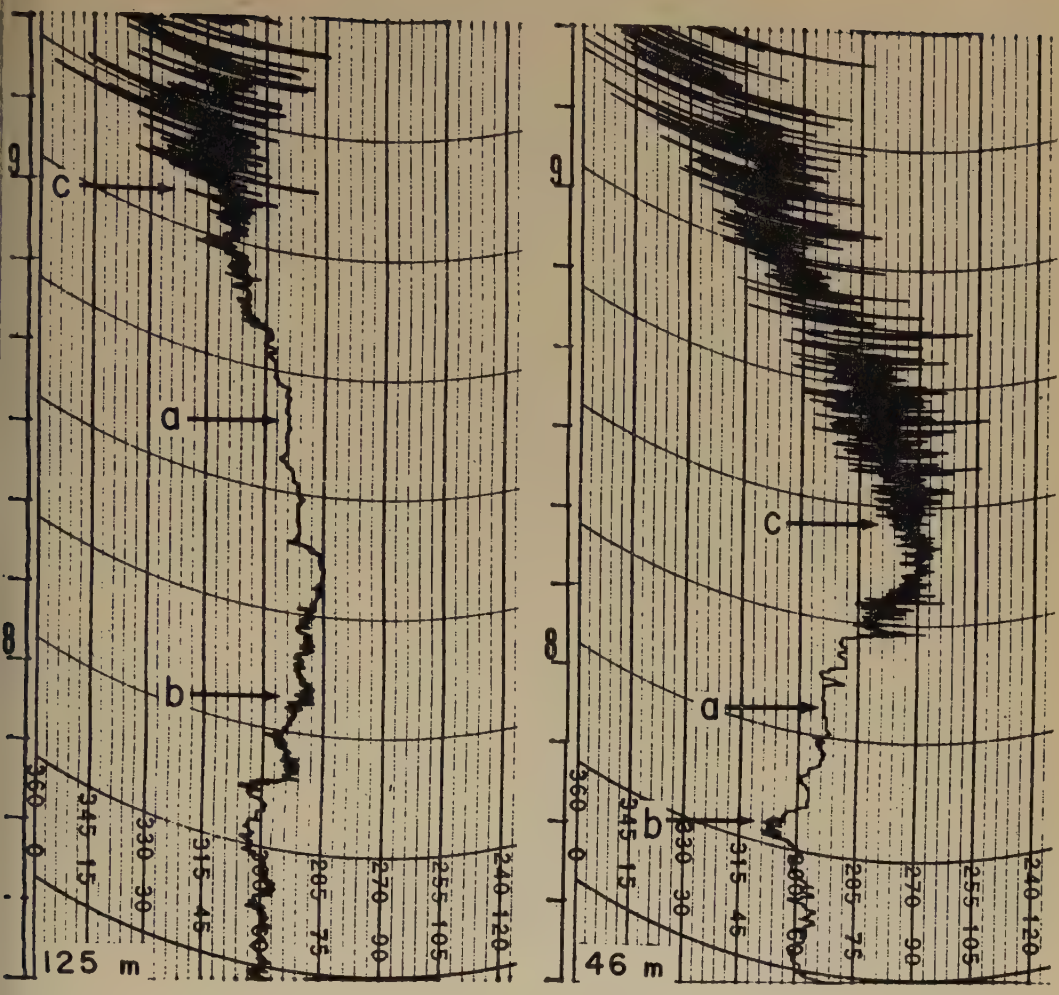

Figure 1. An example of changes in the character of atmospheric flow at 46 m and 125 m, Upton, N.Y., 14 Nov. 1950, 0720-0920 hr.

a = smooth flow; b = damped eddies; c = turbulent flow.

The wind direction is in the north-west quadrant. Short horizontal lines at the left side of each record indicate ten-minute intervals.

existence of a critical number which varies with height alone. Since determinations of the critical *Ri* in the atmosphere near the ground have frequently been limited by sparseness and scatter of data in the neighbourhood of the transition to turbulence, the present investigation was aimed at a determination which would, if possible, more closely delimit a critical value.

2. Determination of the critical Richardson number

A value of the critical Richardson number for the onset of turbulence might be computed from the temperature and wind-velocity differences across a thin layer at the time of the transition. However, this procedure demands inordinate instrumental accuracy. If thicker layers and a 'layer' critical number are considered, an additional complication appears. The onset of turbulent flow in general occurs at different times at the bottom and top of the layer. In addition, the length of period over which mean values of the wind and temperature are computed must be settled.

It is readily seen by reference to Fig. 1 that the quasi-periods of the oscillations studied here are of an order less than one minute. Wind records made at fast chart speeds indicate that immediately after the transition to turbulence we are dealing with eddies roughly 15 m to 25 m in size, which correspond to the frictional eddies of Durst (Giblett *et al.* 1932). If it is assumed that the energy of these motions derives entirely from motions of larger scale, then the Richardson number should be derivable from mean values of the wind velocity and temperature over periods that are long compared with those of the oscillations studied. However, means centred upon the time of an abrupt change in turbulence are not satisfying, partly because of the discontinuity at the transition point, and partly because the transition is not in general simultaneous throughout the layer.

An indirect approach to the determination of the critical *Ri* was therefore adopted. In general, two ' layer ' Richardson numbers are computed for each case studied, one from mean wind speed and temperature differences before *any part* of the 79-m thick layer becomes turbulent, and the other, after the *entire* layer becomes turbulent. Ten-minute means are assumed to be long enough in relation to the period of turbulent oscillations present. Use of wind-speed differences instead of velocity differences is justified if the change of wind direction with height is small. Mean values of the wind speed and temperature differences are therefore determined before transition occurs at the bottom of the layer, and after transition occurs at the top of the layer. Thus, the critical value for the layer should be bounded by the two Richardson numbers so obtained. Furthermore, this (layer) critical value may be expected to approximate to the critical *Ri* defined in terms of infinitesimals at some height within the layer.

3. Source of data

The method described above was applied to wind and temperature data recorded at Brookhaven National Laboratory, Upton, Long Island, New York, during the months October and November 1950. The site has been described by Lowry *et al.* (1951). An elevated layer was chosen for study in order to decrease possible unwanted effects from failure of the wind vane as a turbulence indicator in nearly calm conditions. The layer 46 m to 125* m was selected because continuous records of temperature difference were available, and because especially suitable wind instruments were installed at these two heights. Only data at sunrise were processed, since the transition from smooth flow, or flow with damped eddies, to turbulent flow is then usually regular and readily recognized. In two cases when the transition was not abrupt enough at either the bottom or top of the layer to permit choice of a definite time, only one pair of wind and temperature differences was used.

The temporal course of the wind direction was obtained by means of standard Aerovane wind systems. According to Mazzarella (1952), the damping constant of the vane is 0·042 sec^{-1} at a wind speed of 1 ft sec^{-1}, and is proportional to the wind speed; the free period is 40·3 sec at 1 ft sec^{-1}, and is inversely proportional to the wind speed. A separate, special Aerovane system provided the wind movement at 125 m and the difference in wind movement between 46 m and 125 m, over successive ten-minute time intervals. When the transition took place during one of these prearranged time intervals, as generally happened, mean wind and temperature differences were determined from the preceding (before transition) or following (after transition) ten-minute periods.

Mean temperature differences were graphically obtained directly from continuous temperature-difference records. These were provided by a system consisting of resistance thermometers (Thermohms) at 46 m and 125 m, and a recording Wheatstone-bridge. One Centigrade degree corresponded to a radial chart distance of 4·5 mm. Both thermometers were mounted inside aspirated radiation shields. The time-lag constant of these aspirated resistance thermometers is 72 sec (Mazzarella 1953). Five water-bath calibrations of the temperature-recording system were made between 28 September and 17 November, and interpolated corrections were applied to all temperature data.

4. Results

Fig. 2 presents the results of fourteen transitions to turbulence of the type described and illustrated in the introduction, in which the change in turbulence was abrupt enough to be clearly recognized. The observed temperature differences across the layer 46 m-125 m are plotted

* The height interval of temperature measurement was actually 45·0 m to 124·2 m; of wind measurement, 46·6 m to 125·9 m. Wind and temperature instruments were mounted on booms that extended about 6 m west-southwestward of the supporting tower.

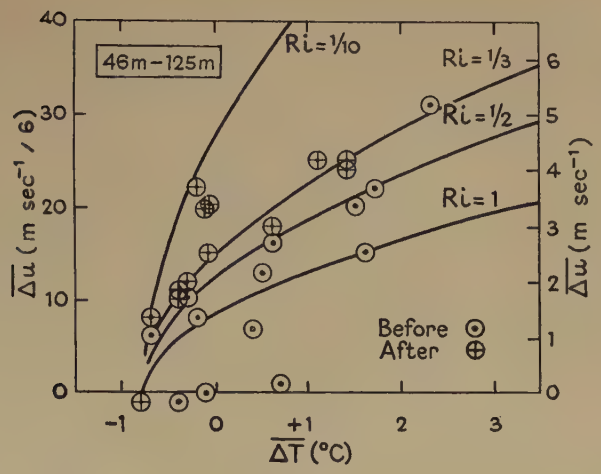

Figure 2. The critical Richardson number indicated by values before and after transition to turbulence.

against the observed wind-speed differences. Lines of equal (layer) Richardson number Ri are drawn for the values 1/10, 1/3, 1/2, and 1, by :

$$\overline{\Delta T} = (0{\cdot}010)\,(\overline{\Delta u})^2\,(Ri) - 0{\cdot}78,$$

derived from the expression given earlier for the Ri where $\overline{\Delta T}$ is the mean temperature difference in °C, and $\overline{\Delta u}$ is the mean wind-speed difference in units of m sec^{-1}/6. A mean value of 288°K for T was used in computing the constant, 0·010. The change of wind direction with height was generally less than 15°.

Dots within circles represent 10-minute mean values of wind speed and temperature differences during smooth flow, or during flow with damped eddies, before the transition to turbulence took place at 46 m. Crosses within circles represent the 10-minute mean values during turbulent flow, after the transition to turbulence had taken place at 125 m. The critical Richardson number is expected to lie between the upper bound of data referring to smooth flow and the lower bound of data referring to turbulent flow.

The transition at the upper level took place after the transition at the lower level in all cases. Mean wind speed at 125 m during the fourteen transitions studied ranged from 3 m sec^{-1} to 8 m sec^{-1}; the distribution of mean wind directions at 125 m covered all quadrants.

5. Conclusions

Considered by themselves, these results do not, of course, verify the form of the criterion found by Richardson for increase of turbulent energy. However, if Richardson's criterion is in the correct form, the present results indicate that, for the elevated layer studied, it is very likely that the (layer) critical values lie between 1/3 and 1/2. This result is limited to those eddies with periods of order less than one minute, but sufficiently long to influence the wind vane, i.e., to what Durst called frictional eddies (see Giblett *et al.* 1932). In the light of the recent work of Panofsky and McCormick (1951), eddies on this scale may contribute significantly to the total vertical flux of momentum and heat.

The comparatively narrow range indicated by these results invites application of the technique to eddies of smaller or larger scale, using appropriately different meaning periods. Paeschke's results referred to earlier would indicate that if sufficiently small scales are treated, a point would be reached where the technique would fail, or at least require new definitions. On the other hand, if larger scales are treated, difficulties may be expected when scales at which energy is introduced into the flow are approached, and the assumption that the flow is characterized by mean values of the parameters on a scale larger than that of the disturbances no longer holds.

6. Acknowledgments

Preliminary investigations were sponsored by the U.S. Weather Bureau. Appreciation is expressed to Prof. O. G. Sutton, who recommended investigation of the Richardson number as one of several promising lines for research in his lectures on atmospheric turbulence at the U.S.

Weather Bureau in October-November 1948. Helpful discussions with Prof. H. A. Panofsky and Dr. H. Wexler, the assistance of Mr. M. Lefkowitz and Mr. A. J. Belfour in the compilation and treatment of data, and the cooperation of members of the Meteorology Group at Brookhaven National Laboratory are also gratefully acknowledged.

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A further study of the upper westerlies; the structure of the wind field in the eastern North Atlantic and western Europe in January 1950

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(Manuscript received 13 March 1953)

Summary

(a) A study of 62 vertical cross-sections through the middle-latitude wind fields of January 1950 confirms previous conclusions regarding the broad structure of the upper westerlies.

(b) A mean January section from Greenland to the Mediterranean contains a wind maximum of 60 kt at a height of 9 km, 61°N. Examination of mean winter-flow patterns for the North Atlantic shows the mean south-westerly jet stream to be a normal seasonal feature.

(c) Comment is made on two features which require further study : high-speed currents in the Arctic stratosphere and low-latitude jet streams.

1. Introduction

Notes on the upper westerlies of September 1950 have been presented in a previous paper (Murray and Johnson 1952). A further month's data have been subjected to a similar analysis in order to determine to what extent the conclusions of the earlier work are of general application. The principal results are discussed below.

The weather of September 1950 was predominantly westerly in character over western Europe. January 1950 was selected for the second examination since, during that month, the eastern Atlantic and western Europe experienced a variety of winter types of weather, including a spell of easterlies. 62 vertical cross-section charts depicting the fields of wind and temperature along fixed lines of section from Greenland to the Mediterranean and from the Azores to the Baltic were constructed from the daily 1400 GMT upper-air data. Details of the method of construction of the cross-sections and a diagram showing the lines of section are contained in the earlier paper.

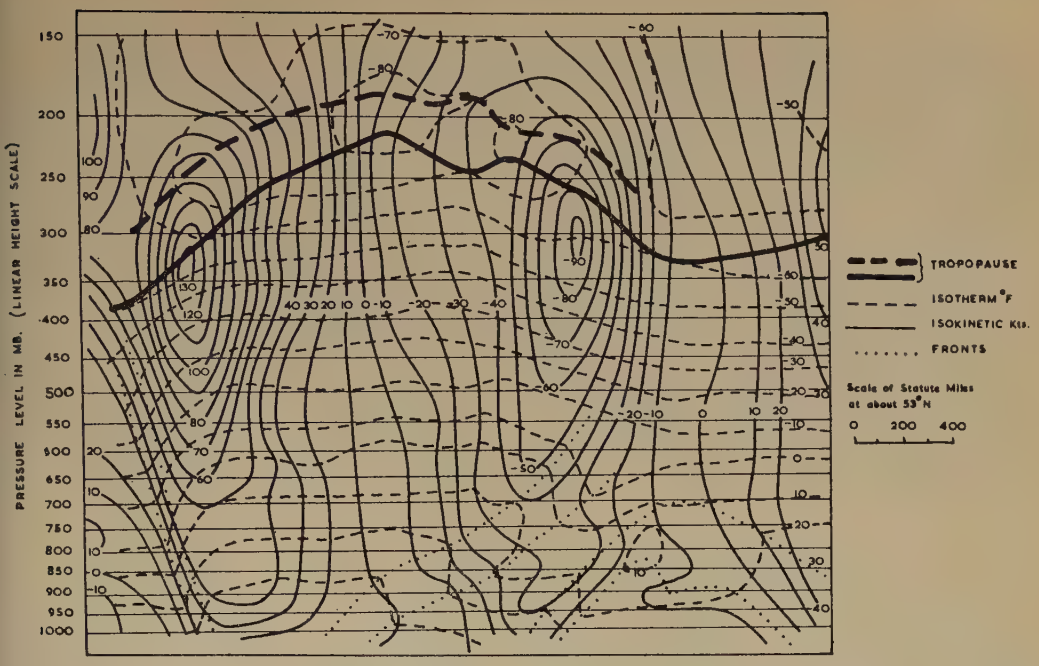

Figure 1. Cross-section Angmagssalik-Tunis, 1400 GMT, 19 Jan. 1950.

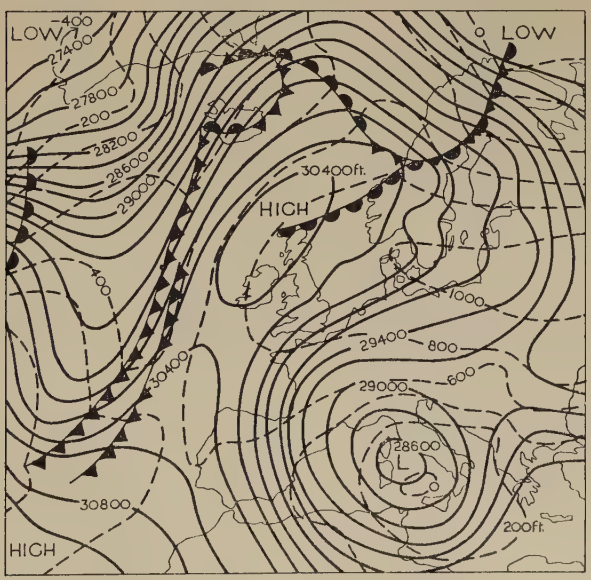

Figure 2. Contours of the 1,000-mb and 300-mb surfaces, 1400 GMT, 19 Jan. 1950.

2. General features

It was previously found that the upper westerlies always contained wind maxima of the jet stream type. This is equally true for the winter month. All the upper currents studied, whether they are major or minor branches of the westerlies or large-scale eddies detached, or partly detached, from the main stream, have essentially similar features in cross-section, namely a central maximum located near the tropopause in association with a baroclinic zone of the type previously described.

For the September study, illustrations were given in which most of the flow was concentrated in a jet stream which occupied one-half to one-third of the section. This was again the most frequently occurring type of section. Fig. 1 contains an example of a less common kind whilst Fig. 2 shows the corresponding surface and upper-flow patterns. On this occasion the section Angmagssalik-Tunis intersected a large-amplitude distortion of the westerly current of the northern hemisphere, which affected Europe during the latter part of the month. The warm air thrust up over the north-east Atlantic and the Norwegian Sea is bounded to the north by a stong south-westerly current and to the south by a weaker, easterly jet stream. Sloping across the British Isles is a broad almost isothermal layer which separates maritime from continental air, whilst a shallow tongue of colder continental air has penetrated across southern France and the Mediterranean. The warm-air ascents indicate a double change of lapse in the region of the tropopause.

Over the area covered by the sections, there were currents with central speeds in excess of 100 kt on every day of the month. On 14 days there were jet streams of 140 kt or more. The space distribution of the wind maxima found throughout the month along each section is illustrated in Figs. 3 and 4, which give the positions and true wind speeds (not merely the resolved components) of the core of each individual current. As previously noted, the heights of the maxima tend to increase with decreasing latitude. The frequency with which upper currents with a north-easterly component occurred was about 20 per cent as against 10 per cent in the September study.

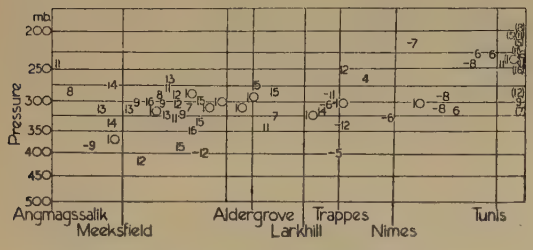

Figure 3. Section Angmagssalik-Tunis.

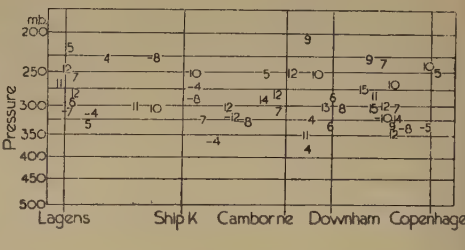

Figure 4. Section Lagens-Copenhagen.

Figures 3 and 4. Positions and magnitudes of centres of maximum velocity in airstreams crossing the sections Angmagssalik-Tunis and Lagens-Copenhagen daily at 1400 GMT in Jan. 1950. The figures give the velocity at the centres of the maxima in tens of knots (positive for flow from the Atlantic through the sections to Europe). Figures in parenthesis refer to currents flowing to the south of Tunis and give the greatest wind speed at the edge of the section (not at the jet centre).

Table 1 shows the number of maxima occurring within given pressure intervals for each line of section. Over the British Isles the jet centres were usually found between 250 mb and 350 mb, a range 50 mb lower than that given for Sept. 1950.

TABLE 1. THE VERTICAL DISTRIBUTION OF THE WIND MAXIMA

	Pressure interval in mb								
	200-225	225-250	250-275	275-300	300-325	325-350	350-375	375-400	400-425
Angmagssalik-Tunis	1	5	7	11	18	8	2	3	1
Lagens-Copenhagen	2	6	8	11	16	6	2	0	0

The relationships between jet streams and surface fronts were found to be as diverse in January as in September. On some occasions the general pattern of the upper flow could be inferred from the surface configurations, on others this was impossible.

3. Observed velocity profiles

The wind shears along an isobaric surface which occurred at the levels of the jet stream centres are illustrated by the velocity (component) profiles given in Fig. 5, which contains curves applicable to type C and type W jet streams (as defined by Murray and Johnson (1952)) in addition to the overall mean profile. The profiles were constructed only from jet streams which flowed almost normally to one of the lines of section.

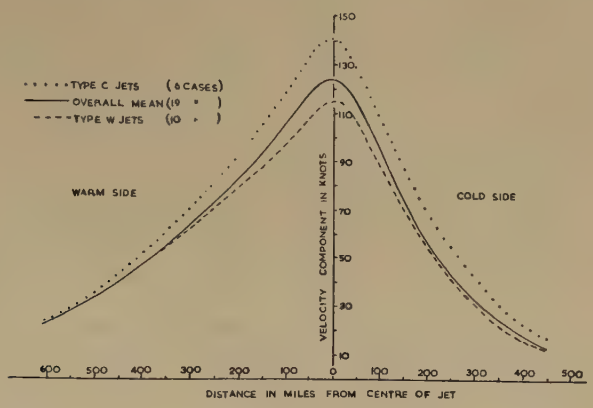

Figure 5. Mean velocity profiles at the levels of the centres of jet streams. Type C jets are those which tend to move laterally towards the warmer tropospheric air, and type W jets are those which move towards the colder tropospheric air.

The three velocity profiles are very similar to each other and to those obtained for Sept. 1950. The speed at the centre for the overall mean is 124 kt as against 96 kt in September, whilst the difference in speed between type C and type W jets is 26 kt in January as against 20 kt in September, type C jets being the stronger in each month. It was previously noted that the average shear on the warm side of the current was about two-thirds of that on the cold side. A similar ratio holds for the January mean profile, the wind speed falling off to half its maximum value in 300 mi and 180 mi on the warm and cold sides respectively. The greatest cyclonic shear of 40 kt/100 mi and the grestest anticyclonic shear of 25 kt/100 mi both occur at about 100 mi from the centre of the mean curve.

The individual velocity profiles from which the mean profiles were constructed were all of the same general form as the mean (cases of double maxima being specifically excluded), although there was considerable variation in the relative intensities of the cyclonic and anticyclonic shears. In no case did the anticyclonic shear exceed the cyclonic shear. The greatest observed cyclonic shear was again of the order of 100 kt in 100 mi, 100 mi being a lower limit to the distance over which it is possible to estimate isobaric shear with this network. Recent experiments in photographic reconnaissance by Hurst (1952) suggest that shears of up to twice this value may occur over shorter distances. Hurst's data give velocity profiles which are very similar in shape to the mean profile obtained from this analysis.

The remarks of sub-section 4 (b) of the earlier paper concerning vertical wind shears apply equally to the jet streams of January 1950.

4. A mean January cross-section

Fig. 6 contains the mean cross-sections from Angmagssalik to Tunis for Jan. 1950. Mean values of temperature and normal wind component were computed from the individual sections over a network of points at vertical and horizontal separations of 3,000 ft and 150 mi. It is not possible to represent the tropopause on the mean section with entire satisfaction. On the cold side of the mean current the level of transition from mean troposphere to mean stratosphere can be placed within fairly narrow limits from suitably constructed mean ascents. On the warm side, however, the placing of the mean tropopause becomes more arbitary. Following Hess (1948) and Hutchings (1950), the mean warm-air tropopause has been placed at the level of minimum temperature, although it is clear from the mean ascent curves that this level will not correspond with the statistical mean of the individual tropopause heights.

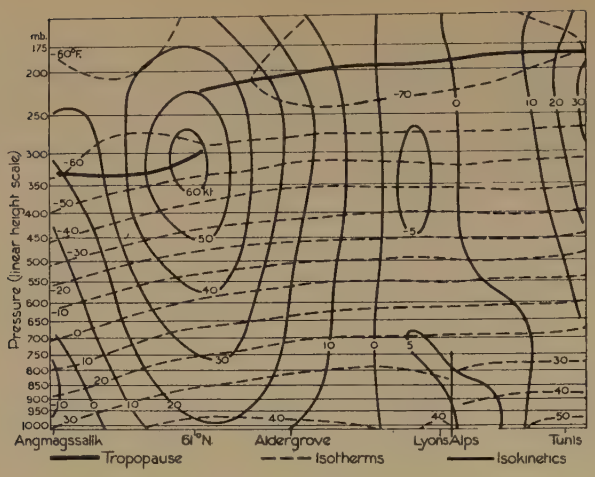

Figure 6. Mean cross-section Angmagssalik-Tunis for Jan. 1950 (1400 GMT).

(a) *The North Atlantic winter jet-stream.*

The mean wind field of Fig. 6 has a maximum of 60 kt at a height of about 9 km in Lat. 61°N. This mean current, although of lesser intensity, is broadly similar to many individual jet streams and is located in a position favoured by the strong upper westerlies on many days of the month (see Fig. 3).

The latitude of the mean current is higher than those of wind maxima found on mean sections published for other longitudes of the northern hemisphere. Hess (1948), in his winter section for 80°W shows a maximum of 50 m/sec (100 kt) at a height of 12 km in Lat. 35°N. To obtain some idea of the representativeness of the January 1950 section, a corresponding mean winter section was constructed from the mean flow charts given by Brooks *et al.* (1950). This is reproduced in Fig. 7. The scale of the mean flow charts as published is too small for the pattern of Fig. 7 to be correct in detail but the broad features are given with sufficient accuracy. Comparison of Fig. 6 with Fig. 7 indicates that the mean flow pattern for January 1950 over the North Atlantic is quite typical of the winter season and that it is normal for a mean south-westerly winter current to be found in rather high latitudes. It is well known that the 80°W meridian lies near the axis of a quasi-permanent cold trough; the Atlantic section is orientated normally to the south-westerly flow on the western flank of a mean warm ridge.

The height difference between the 80°W and the North Atlantic mean currents is a reflexion in the mean of the tendency for individual currents to flow at higher levels in low than in high latitudes. The greater mean speed of the 80°W current is presumably due in part to a greater relative strength of the individual 80°W jet streams (the strongest upper flow is often concentrated to the south of an upper trough) and partly to the lesser variability of the flow over eastern North America noted by James (1951) : the upper flow apparently enjoys a greater degree of freedom over the east Atlantic.

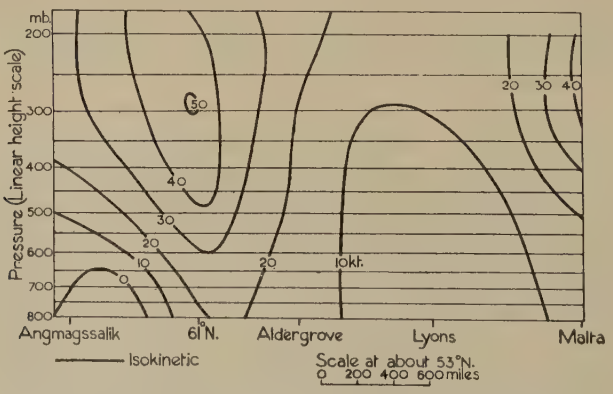

Figure 7. Mean winter cross-section Angmagssalik-Tunis.

(b) *Currents in the lower stratosphere*

To the north of a (westerly) jet stream, there occurs in the stratosphere a band of relatively warm air and further north still, in mid-winter, the winds are often found to increase with height in the lower stratosphere. It is not surprising, therefore, that in the northernmost part of the mean January 1950 section, the wind increases with height both below and above the tropopause. On a few occasions, usually when a jet stream was located in high latitudes, the wind reports from Angmagssalik suggested the presence of a separate maximum in the Arctic stratosphere (see for example Fig. 1). It seems that on some occasions a high-speed stratospheric current occurs in association with the strong thermal gradient which must exist between the warm band to the north of a high-latitude jet and the intensely cold arctic stratosphere noted by Palmén (1948). The effect could not be studied in detail since it occurred at the extreme edge of the fixed section.

(c) *Low-latitude jet streams*

The evidence in the mean for a high-level, low-latitude, wind-maximum is of interest in view of Palmén's suggestion that there exists a quasi-permanent subtropical jet stream encircling the hemisphere as distinct from the more mobile westerly jets of higher latitudes. An examination of the daily sections in conjunction with high-level contour charts showed, however, that the Mediterranean currents were much more variable in position and intensity than the quasi-permanent winter jet stream which flows south of the Himalayas and into which they appear to feed. On most occasions when a jet stream appeared in the extreme south of a section, it clearly originated in higher latitudes, either branching from the middle-latitude westerlies over the North Atlantic, or forming the southern extremity of a large wave-like distortion of the upper flow, such as that illustrated in Fig. 1.

Lack of data prevented the extension of the sections into lowest latitudes and in the absence of such data it is not possible to form a definite conclusion regarding the existence of a semi-permanent westerly current to the south of the Mediterranean and westwards.

Acknowledgments

I wish to thank Mr. J. S. Sawyer and Mr. R. Murray for their continued interest and advice. Acknowledgment is made to the Director, Meteorological Office, for permission to publish this paper.

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The profile of a jet stream observed 1 September 1952

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(Manuscript received 24 March 1953)

In a previous note (Hurst 1952) the author has described a technique for obtaining the winds at short intervals across jet streams and a flight is described which was made at 300 mb on the polar or cold side of a jet stream in January 1952. On 1 September the opportunity arose to make a similar flight across the axis of a jet stream so that the structure of the 300-mb wind, on the warm as well as on the cold side, could be built up. At 06 GMT on that day a cold front

extended from south Denmark to Belgium and north-west France and was moving slowly southward over France. An anticyclone to the south-west of the British Isles was gaining in intensity, and subsidence was effective over southern areas. Winds from a general direction of 260° were reported over the British Isles as a whole at 09 GMT, with a variation in speeds, at 300 mb, from just over 40 kt in the extreme south (Larkhill and Camborne) to well over 100 kt in the latitude of south Scotland (Leuchars 120 kt and Aldergrove 105 kt) and to well below 60 kt in the extreme north (Stornoway and Lerwick). The expectation of well broken cloud in the south, and breaks between showers in the north appeared good grounds for arranging a flight of the same character as before. Accordingly an aircraft was detailed to fly, at an indicated height of 30,000 ft (almost exactly 300 mb) from Bedford on a course 335° (magnetic) which, it was expected, would bring the aircraft close to Cape Wrath in the extreme north.

The flight went as planned; the first vertical photograph at 1045 GMT was over the centre of Bedford, and the Scottish coast was crossed, 5 mi S.W. of Cape Wrath, at 1220 GMT. Cloud was well broken in the south, but further north extensive banks of convective cloud were encountered, and there were three somewhat lengthy intervals when no ground detail could be seen (19 mi near Hexham, 30 mi over the Cheviots and 17 mi just to the south of Cape Wrath). As in the earlier flight, the winds have been measured over five-print intervals as far as possible, but deviations were inevitable in many places due to irregularity of ground fixes, and the average interval over the flight was 6 to 7 prints. Slight adjustment was made, as necessary, with ground fixes if they did not seem in harmony with their immediate neighbours.

Fig. 1 has been drawn to show the winds which obtained at the 300-mb level. Continuous lines signify winds derived from photographs and pecked lines those from Gee fixes. It will be seen that the maximum wind calculated was 112 kt at 55° 50′N.; it is unfortunate that the major gap in fixes due to cloud cover occured in just this vicinity, so that the peak wind might well have proved 115 kt, or even 120 kt. The shear of wind on the cold edge of the stream was from 112 kt to 45 kt in a distance of about 75 n mi (a gradient of 67 kt/75 n mi, compared with the stronger January value of 90 kt/80 n mi). From this point, 75 n mi north of the axis

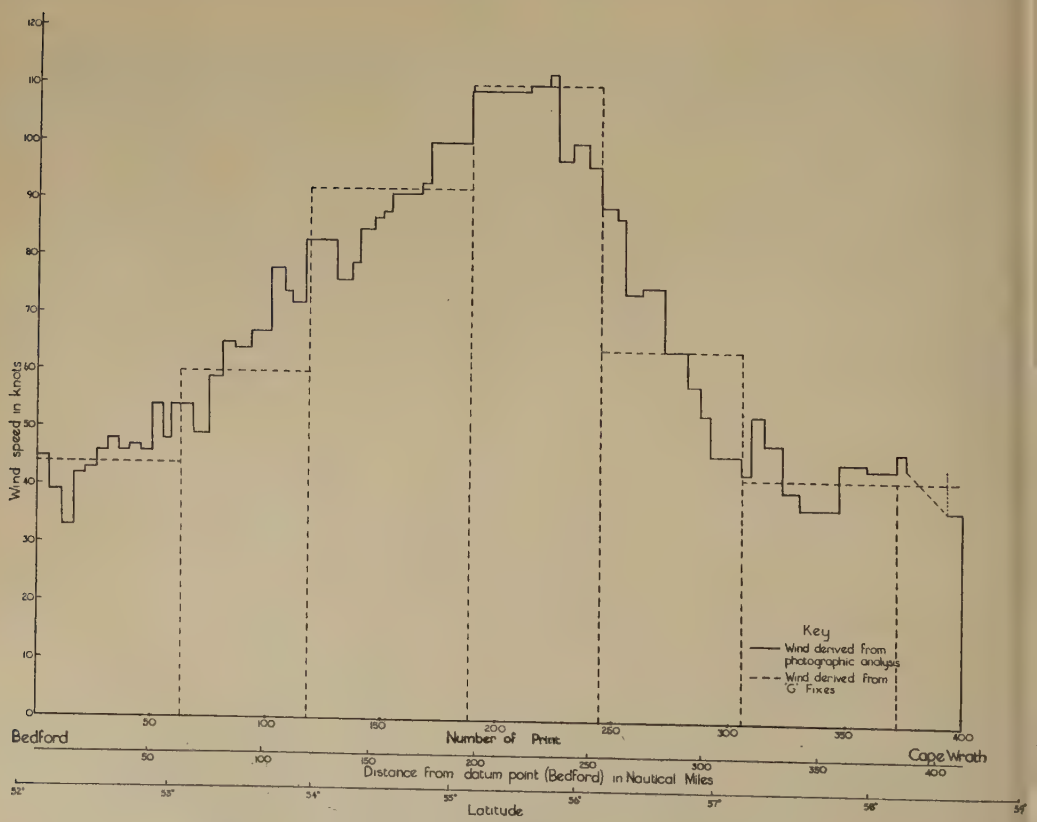

Figure 1. Variation of wind strength with latitude and distance.

of the jet stream, wind remained almost steady at 45 kt for some distance; there is a break in the run from 58°15′ to 58°30′N, and in the extreme north the measured wind was 39 kt. The Gee-winds over the whole of this part of the flight were rather stronger than 40 kt. On the warm edge, the shear was more uniform and of course less intense, and, from the start of photography until the jet axis was reached, the wind increased from 40 kt to 112 kt (a gradient of 72 kt/200 n mi). It will be noted in Fig. 1 that agreement between winds derived from photograph analysis and from Gee fixes is very close. It is of interest that the maximum value of the ratio of the anticyclonic shear to the Coriolis parameter was 0·83, so the possibility of dynamic instability does not arise; this of course agrees with theoretical findings of Sawyer (1949) and others. The ratio of cyclonic shear to Coriolis parameter has a maximum value of 1·75.

In view of the somewhat irregular nature of the wind profile in Fig. 1, an assessment has been made of the errors involved in wind computation. Errors can arise from three main sources :

(a) Errors in locating print centre.
(b) Variation of the attitude of the aircraft during flight.
(c) Variation of the heading of the aircraft during flight.

Of these, (a) is almost negligible. Prints can be identified and marked on a one-inch Ordnance Survey map to an accuracy of about 1 mm, and over a series of 6½ prints the probable error is 0·9 kt.

Errors arising from (b) are due to yawing, pitching and rolling. Of these, yawing does not affect the centre of the print, and the effect of pitching is primarily on computations of wind direction and only secondarily on wind speed. Rolling can however affect the deduced wind significantly. It is an almost instantaneous phenomenon (a matter of, at most, a few seconds) and, according to the pilot, its amplitude should not exceed 2°. This extreme value could give rise to a ground displacement of about 0·20 mi, and a maximum error in wind speed over 6½ prints of 12 kt. This error has been largely eliminated as correction has been made before taking measurements on any fix which was plainly out of line both with the track and with its immediate neighbours. It is conceivable that some element of real wind deviation may exist in such cases. About half the fixes were adjusted slightly, although very few changes of more than 0·1 mi were necessary. It is difficult to say what residual probable error may remain after this correction, but it is thought to be less than 1·5 kt.

The most important uncorrectable source of error is that arising from (c). The pilot considers that the angle between flight and course would very seldom be as great as 2°, and that in most cases the deviation would not be as great as 1°. Accepting 2° as the definition of three times the probable error in this case, errors in wind speed estimation arising from this source would be 3·2 kt.

The combined probable error over 6½ prints, from the above assumptions, is 3·6 kt.

An analysis was made of the profile in Fig. 1. It was found that on the warm side the increase in speed in the print range 60-185 (about 135 n mi) was almost linear and, accordingly, a standard deviation was calculated using the linear assumption ; a probable error of 3·5 kt emerged, and the mean print interval was 6·5. A similar calculation was made on the cold side, and a probable error of 3·1 kt emerged over (fortuitously) the same print interval. As these values are both lower than 3·6 kt, it can be concluded that the winds which were derived are not inconsistent with a smooth curve, and that possible errors due to deviation have in fact been somewhat over-emphasized.

Fig. 2 has been drawn to show the cross-section over the flight area at 15 GMT, the nearest time to that of flight; as many radiosonde and wind ascents as possible have been used by means of projection across the track. Difficulty was experienced in deciding the best angle of projection, and in the end a standard 260° projection (a mean wind) was used for all stations; this was almost perpendicular to the mean line of flight. The wind field was drawn in terms of actual speeds, and not of components perpendicular to track. A compromise had to be made on this point; it was clearly more desirable to use components than actual winds, but, at levels between 600 and 300 mb, a trough with an associated 30° wind change lay between Aldergrove and the flight line, and it was felt that resolution was hardly admissable in such a case. One or two curious points occur in the wind structure; the maximum Leuchars wind in the zone 250-200 mb (well above the tropopause) and the secondary maximum wind speed at Aldergrove at 450 mb are both anomalous. The figure is diagrammatic only as, in view of the difficulties due to varying distance of stations from the track, and the existence of the upper trough, a quantitative

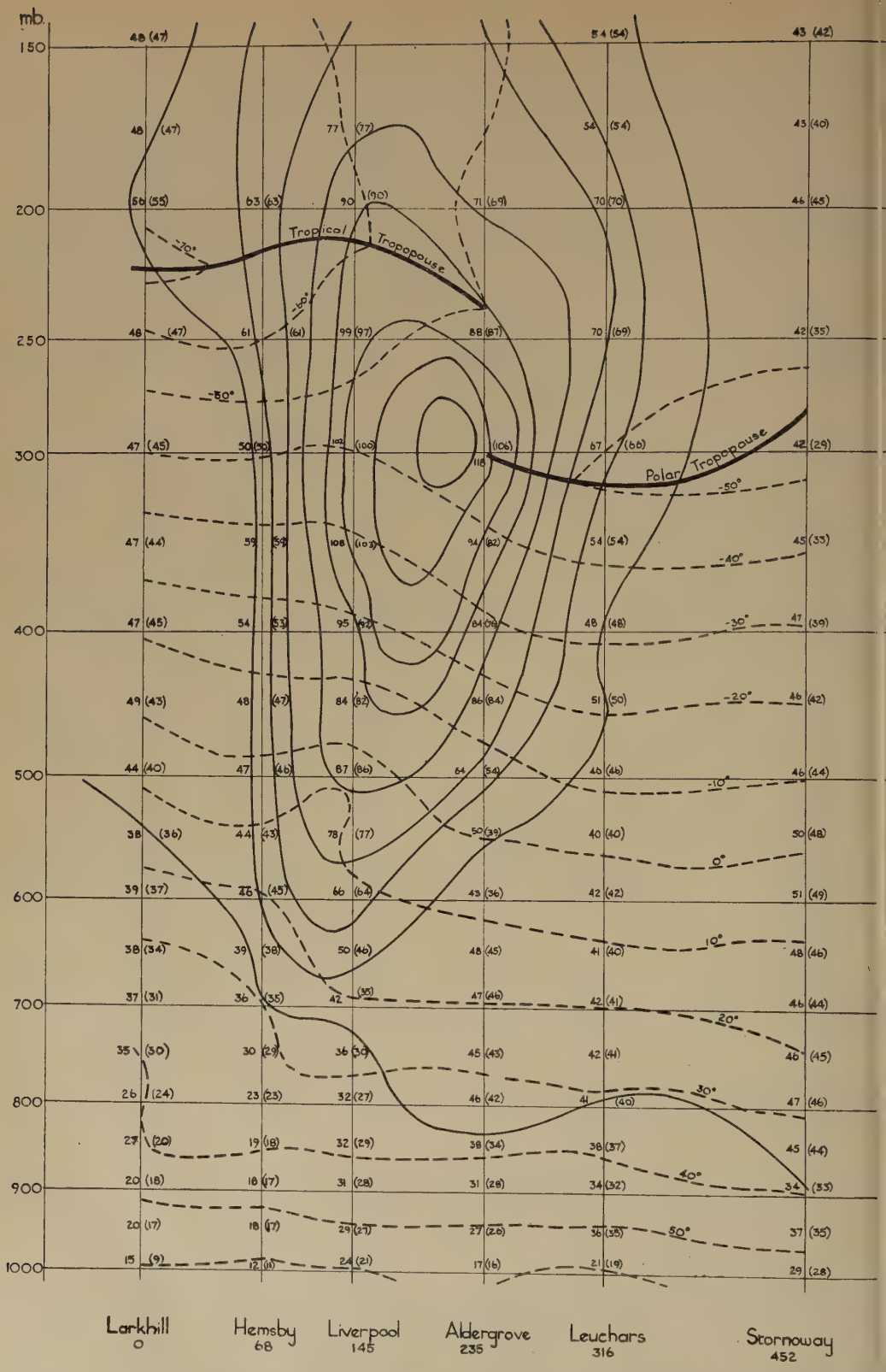

Figure 2. Vertical cross section Larkhill-Stornoway at 15 GMT. Isotherms are shown by broken lines with temperatures in °F entered against them. The speed of the actual wind is shown by the figures at each level above the stations. The figures in brackets show the component of wind at right angles to the plane of the paper.

agreement between the thermal and wind fields cannot fairly be evolved. In particular, it will be noticed that the anticyclonic shear appears as great as the cyclonic shear, although at flight level it was only half as great.

Acknowledgments

Thanks are due to Fg. Offs. F. N. Slaymaker and G. A. Naldrett who undertook the flight. Helpful suggestions were made by Messrs. J. S. Sawyer, D. H. Johnson and P. G. F. Caton. The paper is published by permission of the Director of the Meteorological Office.

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551.511

Organization of kinetic energy realized in the atmosphere*

By D. P. McINTYRE
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(Manuscript received 16 February 1953)

Summary

The methods of Margules and Normand for investigation of the problem of storage and transformation of energy in the atmosphere are briefly described. Through discussion of an example of Margules it is shown that consideration must be given to the manner in which the kinetic energy, created in the process of energy transformation, is organized. In particular it is shown that dynamic factors severely limit the quantity of thermodynamic energy which can be released in the atmosphere.

1. Introduction

In the early part of this century Margules (1906) published his famous paper on the energy of storms and opened a new era of meteorology. It remained, however, for Normand to reveal the true significance of his work. In a series of papers published in the late 1930's and middle 1940's, Normand (see 1946) developed the ideas of Margules and showed clearly how energy problems involving complex systems of air parcels could be solved.

Normand was interested in the quantity of energy which could be wrung from the atmosphere in various particular situations. His attack was thermodynamic and his tool was the tephigram. By computing the changes in stored energy as a set of air parcels were put through their paces it was possible for him to derive the total amount of thermodynamic energy available for development. What he did not consider was the manner in which the kinetic energy, so derived, was organized. Nor did he consider the possibility that this kinetic energy might be related to forces which might prevent the extraction of all of the available energy. It is the purpose of this paper to show that this may indeed be the case and to discuss the manner in which the kinetic energy is organized.

2. Storage of thermodynamic energy in the atmosphere

For simplicity our discussion of this topic will be limited to dry air although, as Normand has shown, the argument may be readily extended to moist air. The specific enthalpy, or enthalpy of a unit mass of air, may be defined as the amount of heat required to raise the temperature

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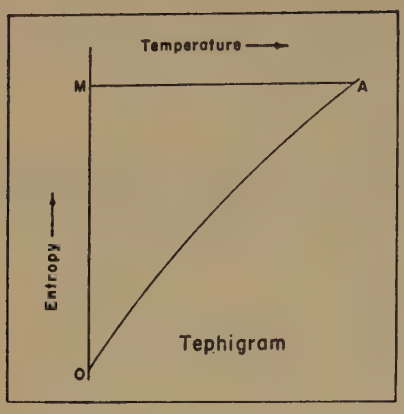

Figure 1. Schematic tephigram. OM and OA, respectively, are isotherms and isobars.

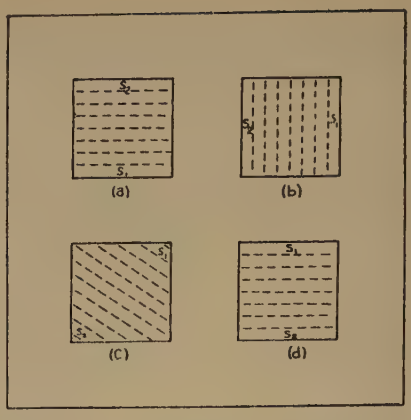

Figure 2. Successive stages in overturning of an unstable air column when entropy $S_1 > S_2$ (after Margules).

of the unit mass from an initial arbitrary zero state, at constant pressure, to the existing state. Thus the enthalpy represents a definite quantity of energy. The importance of enthalpy as a concept lies in the fact that it embodies all of the energy of a parcel of air, apart from the kinetic and the potential. If an element of the heat added is dq we know that $dq = Tds$ where T is the temperature and s is the entropy of the air. From this it is apparent that the specific enthalpy corresponding to some state, say that of the point A on the tephigram in Fig. 1 must be equal to the area OAM. The isotherm OM represents an arbitrarily selected temperature for which the enthalpy shall be considered to be zero. This fact provides a means of computing changes in enthalpy as a parcel, or group of parcels, move according to some plan.

3. An example of Margules

In Fig. 2(a) is shown an example of unstable equilibrium which was first studied by Margules (1906) and later by Normand (1938). The entropy s_1 is assumed to be greater than the entropy s_2 and hence the lapse rate of temperature is superadiabatic. Margules assumed that the air would overturn and in so doing would pass through the stages shown in Figs. 2 (b) and 2 (c) and finally reach the equilibrium position shown in Fig. 2 (d). From this he computed the quantity of enthalpy which would be lost by the system. The enthalpy drop is a function only of the initial and final states and one could assume some other process to produce the overturning. Since it allows for simpler computation on the tephigram, Normand assumed, for his computation, that the upper and lower parcels of air slid past one another. In each case, however, it was assumed that the air column suffered a complete overturning. By equating the energy released to $\frac{1}{2} Mv^2$ the velocity of the resulting air motion could then be computed.

4. Organization of kinetic energy

When one takes into account the manner in which the final motion is organized, it appears that the procedures described in Section 3 for computation of the released energy and the resulting velocity are not realistic. The thermodynamic approach always assumes that energy can be released if the enthalpy of the system is not a minimum. If this were truly the case a situation such as that shown in Fig. 2 (c) could exist only momentarily. Yet strong solenoidal fields such as this do persist for long periods in the atmosphere.

Let us select axes on Fig. 2 such that the y-axis is positive towards the right and the z-axis is positive upwards. The x-axis will then be positive in a direction towards the reader. The vorticity equation for the yz-plane can then be written

$$\frac{d\xi}{dt} + \xi\left(\frac{\partial v}{\partial y} + \frac{\partial w}{\partial z}\right) = f\frac{\partial u}{\partial z} + \frac{g}{\theta}\left(\frac{\partial \theta}{\partial y}\right)_p \quad . \quad . \quad . \quad . \quad (1)$$

where u, v, w are components of velocity

f is the Coriolis parameter
g is the acceleration due to gravity
θ is the potential temperature
ξ is the x-component of vorticity

The two terms on the left, taken together, represent the rate of change of circulation.

Figure 3. Circulation associated with an overturning unstable air column.

In the initial state, see Fig. 2 (a), all of the terms in Eq. (1) are zero. If, however, one imagines a small perturbation which starts a circulation, say in a cyclonic sense, then one finds that the solenoid term (final term) becomes positive as in Fig. 3. From the equation it is seen that this gives a positive value for the circulation acceleration and the initial cyclonic vorticity must increase. As it increases the solenoid term (final term) also increases. And so energy is released at a continually increasing rate. It is therefore apparent how the kinetic energy in the yz-plane is organized. But another factor must be considered. The horizontally moving air, shown at the top and bottom of the box in Fig. 3, is immediately subject to the action of the Coriolis force. This gives components of motion parallel to the x-axis in such a manner that the rotational term (first term on the right-hand side) of Eq. (1) becomes negative. This tends to offset the effect of the positive solenoid term in the equation and reduces the rate of production of cyclonic circulation. The circulation acceleration vanishes when the rotational term and the solenoid term become equal in magnitude, but opposite in sign, i.e., when the thermal-wind equation is satisfied. This point marks the effective end of the process of energy release. The circulation does not immediately cease at this point which is, in fact, the time of maximum circulation. The motion therefore overshoots the mark and an oscillation about the position in which the thermal wind equation is satisfied occurs. This motion would no doubt soon be damped out by frictional forces and only the motion parallel to the x-axis would remain. Regarding the final equilibrium position it is reasonable to suppose that energy release would continue at least until the maximum solenoid field had been developed, a situation corresponding to Fig. 2 (b). From this it appears that the amount of energy actually realizable for this particular example is only about half of that computed by Margules and Normand.

5. Conclusions

While this discussion has been confined to a single example it is apparent that in considering the energy realizable in the atmosphere account must be taken of dynamic factors as well as thermodynamic. Because of the rotation of the earth the final equilibrium position will not be one in which the enthalpy is at a minimum but rather it will be one in which the newly created kinetic energy has organized itself in such a manner that the thermal-wind equation is satisfied.

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1946 *Ibid.*, **72**, p. 145.

551.515.127 : 551.547 : 551.509.317

Some downstream effects associated with large-amplitude troughs in upper flow patterns

By C. V. SMITH and A. G. FORSDYKE
Meteorological Office, Dunstable

(Manuscript received 24 April 1953)

SUMMARY

The change in amplitude of troughs occurring downstream of a (primary) trough in the 500-mb contour pattern over America, is examined for the four days subsequent to the first formation of the American feature.

Over the time interval considered, the association between a large-amplitude primary and the presence, in a broad sector to the east, of a large-amplitude downstream trough is found, in general, to be small. The association increases markedly however when a primary persists for two or three days : on a large proportion of such occasions the next downstream trough attains dimensions comparable with that of the primary. Persistence of this downstream trough would frequently appear to be directly related to persistence of the upwind (primary) trough.

1. INTRODUCTION

In extended (four-day) forecasts prepared experimentally at Dunstable, a qualitative assessment of the future modifications and developments of the 500-mb flow pattern has been a consideration in arriving at the broad-scale evolution of surface systems.

It has at times been suggested in this connection that the formation of a large-amplitude trough should lead to the development of a correspondingly large-amplitude trough downstream. If such a process could be shown to take place within four days of the initial formation, say, of a trough over America, it would obviously have an important bearing on extended forecasts issued for the British Isles. The work reported here was undertaken to demonstrate the general validity or otherwise of the above argument.

2. PROCEDURE

The circumpolar ($1 : 3 \times 10^7$) 500-mb contour charts prepared daily at Dunstable were first examined for the complete year, 1950. Measurements were begun on the occasions on which an approximately sinusoidal trough with axis lying N-S. became first established with given amplitude over America. The amplitude was determined from the central contour (as judged by eye) of the flow pattern round the trough and taken as the number of degrees of latitude from the vertex to the intersection of the axis and the line joining the points of inflection on either side of the trough. Three classes, each covering a range of amplitude, were specified as follows :

Amplitude A, 15° Lat. or more; amplitude B, 10°-14° Lat.; amplitude C, 5°-9° Lat.

The changes in amplitude of the American trough and of troughs present downstream in a sector extending eastwards to 50°E, were examined over the four subsequent days. To avoid overlapping data, initial American troughs of amplitude B were considered only when they formed at least four days before the establishment, or at least four days after the degeneration, of a similar trough of amplitude A.

3. THE DATA

(a) *General results*

Table 1 shows the total of troughs, within the various amplitude ranges, present downstream in the sector on the day of formation (D_0) of the American trough and the distribution of these same features on each of the four subsequent days (D_1 to D_4).

TABLE 1. NUMBER AND DISTRIBUTION OF DOWNSTREAM TROUGHS

Amplitude of downstream troughs	American trough of amplitude A (27 cases)					American trough of amplitude B (21 cases)				
Day:	D_0	D_1	D_2	D_3	D_4	D_0	D_1	D_2	D_3	D_4
A	4	4	6	6	5	5	4	3	3	3
B	25	21	10	12	15	18	16	14	10	9
C	25	20	19	11	8	12	10	8	9	7
X	—	9	19	25	26	—	5	10	13	16
Total	54					35				

X is the number of downstream troughs considered on D_0, which subsequently degenerated into complex or indefinite features (and so were no longer classified), or whose amplitude fell to less than 5° Lat. (the minimum amplitude considered), or which passed eastward out of the sector beyond 50°E.

The number of troughs passing from the sector was at all times small so that few troughs were placed in category *X* for this reason.

Table 2 shows the number and distribution of *new* downstream troughs appearing within the sector over the period $D_1 - D_4$. These features were not present on D_0. *N* denotes the number of occasions on which no new troughs formed; any apparent discrepancy between the total of the new troughs and the total of occasions on which new troughs formed ($27 - N$ and $21 - N$, respectively) is explained by the appearance of more than one new feature on certain days. These new downstream troughs were mostly of small amplitude and short-lived; some such new features appeared on each of the four days.

TABLE 2. NUMBER AND DISTRIBUTION OF NEW DOWNSTREAM TROUGHS

Amplitude of new downstream troughs	American trough of amplitude A (27 cases)				American trough of amplitude B (21 cases)			
Day:	D_1	D_2	D_3	D_4	D_1	D_2	D_3	D_4
A	—	1	1	3	1	1	1	1
B	1	1	2	3	—	—	2	2
C	10	7	5	7	3	5	3	4
N	16	21	20	15	17	15	15	14

The procedure outlined above, when repeated on 1,000-500 mb thickness charts for the same period, gave results which differed only in detail from Tables 1 and 2.

(b) *Persistent American troughs*

The results quoted in Tables 1 and 2 took no account of whether the American troughs were more than passing features or subsequently retained their simple structure (simple structure in this context usually meant freedom from short-wave interference). Since in fact most of the large American troughs underwent some change, some compromise had to be adopted and the following conditions were taken for persistence: the American trough should have amplitude A and simple structure on D_0; subsequently this trough should have amplitude A or B.

Complexities could be present on one day only, provided that the trough had a simple structure and requisite amplitude on the preceding and following days.

Only the first downstream trough was now studied. When the primary American trough underwent some complication which prevented its further classification, the downstream trough previously considered was followed, if possible, for the remainder of the four-day period. If the American trough was regenerated after an interval of complexity, a new downstream trough could be interposed between the two troughs initially studied though this was an uncommon occurrence. In such a case the new trough (now the first downstream trough) was followed.

Table 3 shows the relationship between the persistence of the American trough and the number of occasions, day by day, on which the first downstream trough had amplitude A and amplitude A or B. The cases shown are independent, i.e., results appertaining to American troughs which persisted for four days, have not been included in the results for troughs persisting less than this time.

TABLE 3. Downstream troughs in relation to persistence of the American troughs

Persistence of American trough	Number of cases (American troughs)	Number of occurrences of first downstream trough of : Amplitude A					Amplitude A or B				
	Day :	D_0	D_1	D_2	D_3	D_4	D_0	D_1	D_2	D_3	D_4
D_0 or $D_0 - D_1$	10	1	–	1	–	1	7	7	4	3	3
$D_0 - D_2$ or $D_0 - D_3$	8	1	–	2	3	3	4	5	4	7	7
$D_0 - D_4$	9	2	2	2	4	3	8	5	8	9	8

From Table 3 there is the suggestion that persistence of the downstream trough might possibly be related to persistence of the upwind trough. To elucidate this point, more stringent conditions for persistence were taken. This involved the examination of additional data (September 1948 to March 1952) to obtain sufficient cases for discussion. Conditions for persistence were taken now as follows : the American trough should have amplitude A and simple structure on D_0.

Complexities could be introduced or a decrease in trough amplitude to B take place on one day only, provided that the trough had amplitude A and a simple structure on the preceding and following days.

Downstream, as previously, only the adjacent trough was studied. Results are shown in Table 4 and are, as before, independent. Extension of the period to D_5 and D_6 for the more persistent American troughs (row 3), indicated that the number of large-amplitude downstream troughs decreased on these days, and that the pattern of increase and decrease apparent in rows 1 and 2 was repeated.

TABLE 4. Downstream troughs in relation to persistence of the American troughs (extended data)

Persistence of American trough	Number of cases (American troughs)	Number of occurrences of first downstream trough of amplitude A				
		D_0	D_1	D_2	D_3	D_4
$D_0 - D_1$	15	3	3	5	2	2
$D_0 - D_2$	8	1	1	1	4	3
$D_0 - D_3$ or $D_0 - D_4$	7	1	4	3	5	7

4. Conclusions

The suggestion that a large-amplitude trough over America (at the 500-mb level) tends to be associated with or followed by a further large-amplitude trough downstream finds only limited support. Such a downstream feature occurs on not more than one occasion in three.

If however the American trough is persistent and retains its simple structure, the occurrence of a large-amplitude downstream trough becomes relatively much more frequent. There is then an increasing probability that the next downstream trough will attain dimensions comparable with those of the primary toward the end of the four-day period following the formation of the primary. Persistence of an American trough with simple structure is however an infrequent occurrence.

Degeneration of the American trough tends to be followed within about two days by dissipation of the next downstream trough.

Acknowledgments

The authors are indebted to the Director of the Meteorological Office with whose permission this note is published, and also to Dr. R. C. Sutcliffe for constructive criticism at various stages.

Appendix

The way in which the large-amplitude downstream troughs were formed is of interest synoptically.

Data for the year 1950 showed 33 distinct large-amplitude A downstream troughs present in the sector to the east of the American troughs over the periods $D_0 - D_4$. Of these 22, six came about through simple growth or increase in amplitude of pre-existing features. In most of these cases, the change was from amplitude B to A for one day only. In a further 11 cases, the large-amplitude downstream trough was formed by the amalgamation or *phasing in* of troughs, commonly of different latitudes. The most frequent process was for a trough of a mobile wave train north of the main westerlies (often initiated by the shearing forward of the northern portion of a large meridional trough with an attendant *cut-off* process in the south) to phase in with a trough in the main westerlies. The remaining five cases were associated with *blocked patterns*, either with a retrogression and increase in amplitude of the trough to the east of the concomitant high and low vortices or with the disruption of the block and the absorption of the blocking low vortex by the trough to the east or west.

The impression gained was that large-amplitude troughs over the Atlantic-European sector were more likely to result from the interaction of wave trains of different latitudes than any other process; the large-amplitude troughs resulting from blocking patterns may also at times be regarded as special cases of this process.

Correspondence

551.557.2 (42)

Rainfall at Bidston, 1867-1951

By S. E. Ashmore

I should like to congratulate Mr. G. Reynolds on this rainfall paper (*Q.J.*, p. 137, 1953) : for one thing it caused me to go back and re-read the former papers on Bidston rainfall and enjoy them as in the 1930's.

I feel Mr. Reynolds has somewhat understated the sheltering effect of the Observatory buildings on the gauge. It is only fair to state that Lammert's results (*Met. Mag.*, **77**, p. 206, 1948) refer to a period of merely four summer months; and to apply these to 85 complete years is risky. On visiting the gauge one is impressed by the large buildings so close, and is tempted to wonder if any rain reaches the gauge at all with a SE. wind. Recent work by Rider (*Q.J.*, **78**, p. 97, 1952) shows that there is distortion of the air flow over a considerable distance due to such a small object as a hedge : there would presumably also be distortion of the trajectories of raindrops in the vicinity of a large building. Some idea of the magnitude of the loss of catch might be found by a study of years with an excess of SE. winds, and even of individual occurrence of downpours with a SE. wind such as 8 November 1951.

Mr. Reynolds is quiet about the year 1872. In view of the trouble it caused in the previous investigations, it would be interesting to know if he has omitted or included it in his frequency-curve graduation.

11 Percy Road,
Wrexham,
North Wales.
11 May 1953.

Reply

By G. Reynolds

I agree with many of Mr. Ashmore's views on the exposure of the Bidston raingauge, but with some reservations. Such a remark as ' tempted to wonder if any rain reaches the gauge at all with a SE. wind ' is a gross overstatement, as a catch of 0·51 in. on 8 November 1951, the day of SE. winds mentioned above, clearly demonstrates. The Observatory buildings would admittedly distort the trajectory of the air flow in the vicinity, and it seems to be tacitly assumed that this would reduce the catch. Is there not a case for suggesting that convergence in the lee of the building would increase the catch, and divergence to windward would reduce the catch with N. or NW. winds ? It will be appreciated that the winds will be blowing round the building as well as over.

This gauge has been read daily under the same conditions for over 85 years. If a dozen gauges were erected on the hill within a mile of the Observatory, they would all show small differences amongst themselves in absolute values, due to local peculiarities, but the overall picture painted by one or all of the gauges would be the same as that of the gauge whose records I have reviewed.

It is hardly accurate to state that I have been quiet about 1872, when the year is mentioned specifically on 10 of the 13 pages of the paper. Clearly from Table 9 and the text of Section 7, no year has been omitted from consideration when computing frequency-curve distributions.

Liverpool Observatory and Tidal Institute,
Bidston.
1 June 1953.

551.542

World-wide distribution of pressure

By A. J. Whiten

In his most instructive paper on 'Climatic variation' in this volume (p. 186) Professor Manley reminds us that, 'since the total amount of air is fixed, and has probably remained so since an early stage in geological time, the average barometric pressure over the whole surface of the earth must be always the same.'

I have often wondered whether this assumption is really justified. There must, of course, be variations on a microscopic scale. The fixing of nitrogen by certain forms of vegetable life, and by lightning, not to mention the small amount of oxygen removed from circulation by the oxidation of metallic and other surfaces, are examples of chemical processes, of which there may be very many others, known and unknown; these might cumulatively account for a small variation, and even though they may be held to cancel each other out over a period of time, there must, it appears, be a small variation from time to time.

This variation in the actual quantity of air may be – probably is – completely negligible, but average pressure is not quite the same matter. The amount of water vapour carried in the air at any place varies within very wide limits, and it is difficult to believe that evaporation over some places on the earth's surface is, usually, even approximately balanced by condensation and precipitation in others. I believe that attempts to demonstrate this constant average pressure have always failed, and that the failures have been attributed to the insufficient cover of the earth's surface by the observations plotted. Is there any ground, other than theoretical, on which this assumption can be sustained?

9 Inveresk Gardens,
Worcester Park,
Surrey.
12 May 1953.

Reply

By Gordon Manley

Mr. Whiten has raised a very interesting point. Very small changes in composition will indeed arise as he suggests and for example the protection of large areas by means of an ice cover can be judged likely to give rise to such changes in quite recent geological time; this is of course apart from other minor changes such as those considered by Mr. Callendar in regard to CO_2. No significant change in overall pressure should however arise.

But with regard to water vapour, a small rise in average world temperature should lead to greater evaporation and such changes have no doubt occurred. Would these not however give rise to greater regional variations in surface pressure rather than to a change in average pressure over the whole earth. Can we allow that increase of temperature would also lead to increased oxidation? Some quantitative calculations would be welcome, and Mr. Whiten does a service in bringing the question to our notice. (For evaporation versus condensation, some comments in a discussion on orographic rainfall in *Q.J.*, **71**, p. 41, 1945, will be found interesting.)

Bedford College,
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London, N.W.1.
17 June 1953.

551.558.1

Fair weather cumulus

As already reported on p. 306 of this volume, a joint discussion meeting with the British Gliding Association was held under the above title on 21 January. The opening papers, general discussion and the summing up are given below.

551.558.1

Atmospheric convection; the structure of thermals below cloud-base

By A. H. YATES
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1. Introduction

During the last thirty years, glider pilots have frequently made use of the convection currents in the lower atmosphere and have acquired a considerable knowledge of them. There is, however, no generally agreed picture of their structure. Although the glider pilot is ideally placed to make observations on the size, velocity and temperature of the thermals, because of the low speed and small turning circle of his glider, few observations have been made, perhaps because the pilot is struggling to remain airborne and cannot afford the luxury of deviating from his optimum path for scientific ends. There is no doubt that the sailplane pilot, properly equipped with recording instruments and provided with facilities to restore him to flight should he be forced to land, can collect data which will not be easy to get by other means.

It is helpful to the meteorologist in planning such flight tests if a picture or model of the structure of these ' thermal ' currents can be postulated. The facts collected can then be used to prove or disprove the model. The purpose of this paper is to present such a model based on the evidence available to date from glider pilots.

2. The experimental data

First let us consider what facts are known to glider pilots.

(a) *Size*

A glider pilot is able to circle his glider at an airspeed of 45 mi/hr with 30° of bank, and the circle diameter is then about 450 ft. He finds that thermals above about 1,000 ft from the ground allow him to perform his circles entirely in rising air so that, although thermals may be small below 1,000 ft, they are of the order of 1,000 ft across above that height and may reach 3-4,000 ft across higher up, although they do not always increase in diameter.

(b) *Vertical velocity*

The rate of climb of the glider (indicated quite accurately to the pilot by a sensitive variometer) is equal to the vertical velocity of the thermal less the rate of sink of the glider in still air. The latter is known to be about 3 ft/sec so that the vertical velocity of any thermal flown in is easily measured.

It is found that maximum strengths of thermals vary from almost zero on a dull day to 15 ft/sec on a ' good ' day and that inside cloud the strength is often greater. The strength appears to be fairly uniform within the thermal and seems to vary directly as the ground temperature on any one day; i.e., it is a maximum in the middle of a hot summer's day.

A negative vertical velocity, i.e. a sink, is often found in the neighbourhood of thermals, but is not normally so strong as the upcurrent – nor is it very widespread. The downcurrent often rings the upcurrent, the boundary being a region of turbulence.

There is not much evidence that the upcurrent strength varies with time. Sometimes the variometer reading slowly subsides to zero, indicating that the glider has descended (relative to the thermal) to a region of reduced upcurrent, but on other occasions the reading improves again after a minute or so. Sometimes, again, the variometer reading falls sharply and indicates sink; this may mean that the glider has fallen out of the bottom of a thermal or has flown out of the side.

(c) *Temperature*

Accurate air thermometers are not easily available to glider pilots so that few measurements are made. The available evidence is, however, that the thermal is one or two degrees Centigrade warmer than the surrounding air at all heights. At about the height at which the atmospheric lapse rate becomes less than the dry-adiabatic rate the thermal stops, unless the air has by then become saturated and formed cloud. In summer, many thermals rise to an inversion level and then stop, penetrating only a few feet through the haze.

(d) *Periodicity of thermals at one site*

Gliders are often launched to 500 or 1,000 ft and search for thermals over an area of about $\frac{1}{2}$ mi^2. The thermal frequency is found to depend on the type of soil, wind strength and sun height and, typically, a sizable thermal is released from this area every 5-15 min. Indications of its release are given by swings of the windsock, a change in the windspeed, dust devils, etc.

3. The thermal model in still air

We need a model which will account for the experimental data we have, and which will give numerical results which are of the correct order.

Because of its considerable vertical extent and limited diameter, a ' thermal ' is most easily regarded as a column of rising air a few degrees warmer at any level than the air outside the column. The column is not necessarily uniform, either in this temperature excess or in vertical velocity, and we shall consider it as consisting of a bubble of hot air with a wake of less warm air moving upward at a slower speed than the bubble (Fig. 1). If the bubble had a skin (like a balloon), or a definite boundary (such as is provided for an air bubble in water by surface tension), the wake would be a region of turbulent air formed by the separation of the boundary layer near the maximum cross-section of the hot-air bubble. The wake pattern and the drag associated with it have been thoroughly studied in wind tunnels and water channels. A bubble of hot air has, however, no skin and the wake will thus be augmented by warm air obtained by the constant erosion of the hot-air bubble by the cold air of the environment. As the column proceeds upwards, therefore, the hot bubble is gradually eroded and its heat distributed to the air entrained from the atmosphere. The buoyancy which has been maintaining the vertical motion is ultimately greatly reduced and the column slows unless a further supply of hot air is received from below.

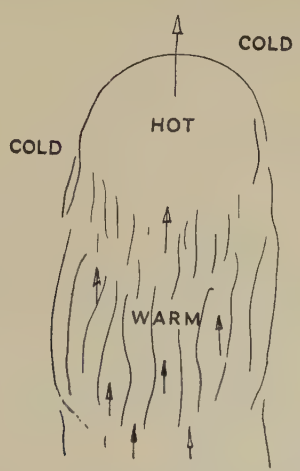

Figure 1. The thermal model in still air.

Before we consider the process of formation of such a bubble, or the effect on it of a horizontal wind, let us examine the probable size of the bubble and its upward velocity. Our quantitative results are bound to be crude because we have no exact measurement of thermal size or temperature excess, but we shall soon see if our model is able to account for the order of sizes and upward velocities we find in practice.

The buoyancy of a bubble of hot air is equal to the volume × density × $\frac{\text{temperature excess}}{\text{absolute temperature}}$ × g, i.e., $\frac{\pi d^3}{6} \rho g \frac{\Delta T}{T}$. The drag force can be estimated, as a first approximation, by assuming that the drag coefficient based on the cross-sectional area of the bubble is that of a sphere (i.e., about 0·5). The drag force is then

$$0{\cdot}5 \times \tfrac{1}{2}\rho V^2 \frac{\pi d^2}{4}$$

The steady upward velocity of the bubble is then given by

$$V = \left\{\tfrac{8}{3}\, dg \frac{\Delta T}{T}\right\}^{\frac{1}{2}}$$

which shows that the large bubbles will rise faster than small ones and with a speed varying as the square root of the excess temperature. For a bubble 1,000 ft in diameter we obtain

$$V = 16\,(\Delta T)^{\frac{1}{2}} \text{ ft/sec approx.}$$

Now this thermal speed is of the right order although rather larger than glider pilots usually find (with ΔT about 1°C a 1,000-ft bubble will often rise at 10 ft/sec, i.e., 7 ft/sec on variometer). By studying the rate of rise of cumulus towers, Ludlam (1952) has found that $V = 6\Delta T$ ft/sec fits his observations as well as any other rule. This implies that the drag coefficient is greater than that assumed above and that it decreases as the speed increases.

Having established that a 1,000-ft bubble would rise at about the speed at which most 'thermals' move, we must consider where this mass of hot air can come from. Radiation from the sun-heated ground warms the layer of air near the ground and gives a layer several feet thick a lapse rate considerably greater than the dry adiabatic. If we assume that a 50-ft thick layer has a mean temperature 2°C above that of the air just above it, then the layer covering a field half a mile square would provide about 3×10^8 ft^3 of hot air. By some process this air is assumed to leave the ground and be diluted in the process by an equal amount of air from outside the super-adiabatic layer, thus reducing its temperature excess to 1°C. The volume is then about 6×10^8 ft^3, which would be just enough to form a spherical bubble 1,000 ft in diameter. This estimate emphasizes that a large area of the ground is involved in supplying the hot air for each bubble so that the 1,000-ft thermal bubbles are not likely to leave the lower layers of the atmosphere more closely than about a half-mile apart (although they may draw closer together during their climb). The frequency with which thermals are released from a given area depends on the rate at which heat is supplied to the lower layers of the atmosphere and this depends on the sun's strength and type of ground.

The process by which the ground layer rises to form the large bubble is not at all clear. If we assume that small bubbles, say 10 ft in diameter, are torn from the ground layer by some disturbance, they will rise rather slowly because of their small diameter. They will be eroded like any other bubble and mix with the surrounding air to form a warm wake. The entrainment of surrounding air will encourage other small bubbles rising close by to enter the rising wake and thus to join forces with the first bubble. In this way bubbles rapidly grow and we can envisage in this way the 1,000-ft bubble being formed by the time the top has reached about 1,500 ft. Thermals found below about 1,000 ft are thus liable to be small and weaker than those found higher up, and this is broadly in line with glider experience. The upcurrent strength should then be steady within the hot bubble and in the wake close behind, but gradually be reduced as the glider sinks through the wake.

How long can a glider remain in the 1,000-ft diameter bubble? At a sinking speed of 3 ft/sec the glider would sink from top to bottom in about 5 min, but in this time the bubble may well have climbed 3,000 ft giving the glider a net climb of 2,000 ft. The thermal does not now cease since the wake is rising at a velocity only slightly less. Sometimes the glider pilot finds that the rate of climb steadily falls to zero, but often it revives again. The explanation may be that another bubble has come up through the rising wake to meet the circling glider.

Figure 2. The airflow due to a rising bubble.

The warm wake will enable later thermals to take the same path as the first with less wastage, since the environment is not only warmer than the free air but is already moving upwards. In this way the leading bubble is constantly reinforced or replaced by succeeding bubbles and the thermal is able to climb higher and higher, past the condensation level until it forms a cumulus cloud. The glider pilot circling steadily in this column will probably not even notice the arrival of reinforcements and may well regard the thermal as a continuous column.

Reinforcing bubbles may also arrive from separate sources nearby. Two gliders are often to be seen, especially near cloud base, circling in separate thermals, each one or two thousand feet across and yet separated by a few hundred feet of air which has not been entrained and may even be sinking. These separate columns often merge until the thermal size reaches, perhaps, 3,000 ft across. On the other hand, at the base of a large cloud there may be several thermal columns entering the cloud with downward moving air between them.

4. Approaching the 'thermal' from outside

If we accept the thermal model of one in each half mile square with a diameter of 1,000 ft, we shall encounter one of these while flying in a straight glide every mile or so. The reaction felt by the glider will depend on the stage of development of the thermal. The first effects felt will be those outside the bubble. A rising bubble will push air ahead of it and outwards and downwards in all directions beside it (the velocities can be estimated by considering the velocities associated with a rising source or doublet).* If the glider enters at A (Fig. 2), the airflow due to the bubble will tend to increase the apparent airspeed of the glider and a slight downcurrent may also be encountered.** The rate of climb will increase sharply after passing through a narrow belt of turbulent air.

If, on the other hand, the glider enters at B, the preliminary downcurrent will be well marked, the turbulent zone wider, and the upcurrent less 'sharp-edged.'

5. The effect of wind on the 'thermal'

If we suppose the wind to have no gradient, the picture relative to a source moving with the air (such as a glider or a cloud) is unchanged; the wake of the bubble remains vertically below it; the thermal below a cloud is vertical. The path of a bubble *relative to the ground* is, however, inclined by an amount depending on the ratio of its rate of climb to the wind speed. For example, successive positions of a bubble and its wake are shown in Fig. 3. It will be seen that the wake of the bubble, including the part nearest to the ground, travels with the bubble and the fact that the base of the wake is constantly passing over new areas encourages the entraining of further bubbles of hot air from the superadiabatic layer.

* A source closely followed by a sink. The flow of a uniform stream past a doublet is the same as the flow past a sphere.

** The upgust which follows shortly has the aerodynamic effect of accelerating the glider which also increases the airspeed (Yates 1951).

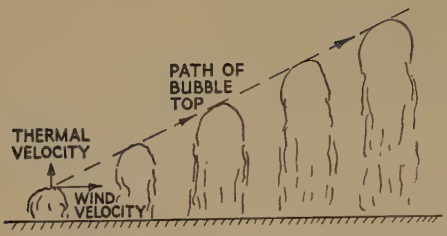

Figure 3. The path of a thermal in the absence of a wind gradient.

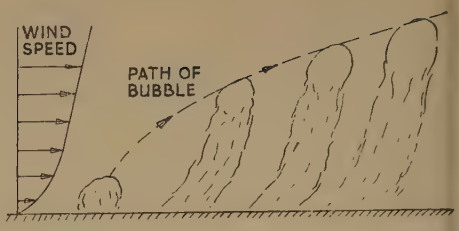

Figure 4. The path of a thermal when a wind gradient exists.

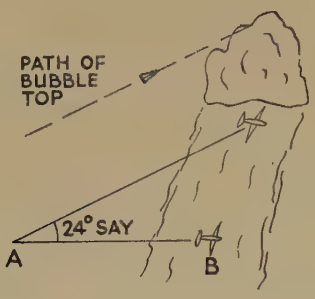

Figure 5. The position of a thermal relative to a cumulus cloud.

In fact, there will be some wind shear and this will result in a leaning of the thermal so that its successive positions will be as shown in Fig. 4. It should be noticed that the inclination of the thermal column results not from the wind alone but from its shear. Rainey (1947) called attention to the fact that the ratio of wind velocity to upcurrent gave actual paths of the bubbles of less than 24° to the horizontal and suggested that ' such oblique angles of ascent may sometimes explain the failure of one sailplane to find lift vertically below another which is rising steadily.' The argument is fallacious although the failure is often true. The lower sailplane should position itself not at A (Fig. 5) but at B. An estimate of the wind shear present at cloud level is obtainable from the shape of the cumulus clouds which are often to be seen leaning.

Another result of a wind shear is that the rising bubble is always moving horizontally a little slower than the wind. As it rises the bubble is constantly moving into layers of faster moving air which exert a drag on it and accelerate it. Ludlam (1952) estimated that in a typical wind shear the bubble (or a cumulus cloud) would be moving over the ground at a speed of 3 to 4 ft/sec slower than the wind at that height. This fact reduces the leaning of the thermal due to shear.

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Rainey, R. C. 1947 ' Observations on the structure of convection currents,' *Quart. J. R. Met. Soc.*, **73**, p. 446.

Yates, A. H. 1951 ' Airspeed fluctuations when entering waves,' *Gliding*, 2, p. 186.

551.524.7 : 551.558.1

Fluctuations of temperature below cumulus clouds

By D. G. JAMES
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Summary

A description is given of one flight below cumulus clouds on 28 Aug. 1952. Small rapid fluctuations of temperature were observed by a sensitive resistance thermometer, records being obtained as a photographic trace. It is shown that, with respect to the frequency and type of fluctuation of temperature encountered, the space between cloud base, i.e., 3,100 ft, and ground level may be divided into three sections. The first, a layer extending from ground to about 800 ft, the second from 800 ft to about 2,800 ft, and the third from 2,800 ft to cloud base. A possible explanation for the formation of these layers is discussed.

1. Introduction

A number of flights have been made by the Meteorological Research Flight to investigate the mechanism of thermal convection over the land and the sea. One such flight over the land is described here. Level runs were made at constant height and airspeed at 500-ft intervals from ground to cloud base, during which time records of small fluctuations of temperature were obtained. These traces are presented in Fig. 1 (Plate III), and a short account is given of the type of variation encountered at each level. The conclusions reached concerning the origin of these temperature fluctuations are necessarily tentative, but several more flights have been made, and it is hoped to have, soon, sufficient material to prepare a more comprehensive account of the mechanism of thermal convection.

2. Flight details

The Hastings aircraft of the Meteorological Research Flight used for this investigation carried five observers together with its normal aircrew of pilot and engineer. Readings were taken every ten seconds, of height, airspeed, temperature as given by the Meteorological Office flat-plate thermometer, and frost point, whilst one observer operated the ultra-rapid thermometer.

This latter instrument (Grant 1950) measures rapid fluctuations of temperature by the variation of the resistance of a pair of fine platinum wires, and has a lag of the order of 5 milliseconds. The amplified response is continuously photographed on a film moving at a rate of 4 in./min whilst the sensitivity generally used $\simeq 1°F/in.$ Note was also made of the cloud directly above the aircraft on its line of flight, by an observer sitting in the second pilot's seat. The first run at 120 kt was performed at 500 ft above ground level and thereafter at 500-ft intervals up to 3,000 ft.

3. The synoptic situation

The flight was carried out from 0930 hr to 1130 hr on 28 Aug. 1952, in the region from Basingstoke to Aldermaston, the pilot keeping as nearly as was possible to this line.

An inactive cold front lying along the English channel at 09 hr was almost stationary. A second cold front, also inactive, lay E-W across northern England and was moving slowly south. An anticyclone over the Azores was extending its influence north-east, giving small rises of pressure over the whole of southern England.

The 02 hr and 14 hr Larkhill ascents indicated considerable subsidence between these times, down to 3,000 ft, whilst the layers below this level also showed much drying out during the morning. An inversion of 1°F between 4,500 ft and 5,000 ft increased during the morning to 4°F between the same levels. The Camborne 02 hr ascent was much moister at all levels, and indicated that a moister air mass was extending itself slowly from the Atlantic across S.W. England. Taking the Farnborough 08 hr observations of temperature and dewpoint, 62°F and 55°F respectively, it was expected, from the parcel theory, that shallow cumulus could be formed with base at about 3,000 ft, there being no upper cloud. During the morning, the cloud increased to 4/8 cover with its base lifting from 2,500 ft, but there was never more than 4/8 cover during the whole day.

The winds reported at Farnborough were 5 kt to 10 kt, with a direction fluctuating between 280° and 320°. The 09 hr Larkhill 'Pilar' showed winds veering from 290°, 6 kt at the surface to 314°, 10 kt at 2,000 ft, then a gradual backing to 296°, 6 kt at 5,000 ft.

The area over which the flight was made had a surface temperature which rose from 64°F to 69°F during the period of the flight, whilst the surface dewpoint dropped from 53°F to 50°F. From these observations it seems that the development of existing cumulus clouds decreased rapidly between 0930 hr and 1130 hr, the time of the flight.

4. Description of the traces

(a) 500 *ft*

As is to be expected, the temperature fluctuations recorded inside the friction layer are large and well defined. The pulses seen show, in general, a temperature increase of 0·6°F to 0·8°F, but there seems to be no preferred diameter, since their lengths seem to vary from 400 ft to 4,000 ft. The larger bubbles, if indeed these are what are responsible for the temperature fluctuations, were not as common as the smaller ones, but this fact is probably controlled by the type of surface below. The pulses observed are not well-defined square waves. Their edges are sharp, but their interiors are regions of small, intense fluctuations of temperature, up to 0·3°F in the largest pulse.

One pulse, some 4,000 ft across, was observed below a large well-defined cumulus cloud whose base was about 3,000 ft. There may or may not be any significance in this observation although under existing synoptic conditions, the wind shear in the vertical was very slight, but if the path of any large bubble is a preferred path for future bubbles then it is possible that this observed large bubble was rising to assist the development of the cloud directly above. The phenomenon has been noticed on several other flights, including cases when the vertical shear was considerably greater.

(b) 1,000 *ft*

For all the traces seen here, the sensitivity controls on the amplifier were not changed, so that direct comparison is possible between the respective traces. This record shows a remarkable change from that at 500 ft. There is a slow variation of ambient-air temperature, broken by sporadic outbursts of small, rapid fluctuations of temperature. These are of the order of 0·2°F to 0·3°F, and last over distances ranging from 300 ft to 1,000 ft. It can be seen that there is a small temperature difference across these outbursts, and this will be observed at other levels up to cloud base. An explanation of this phenomenon will be offered later.

(c) 1,500 *ft*

Once again the trace is in general quiet, with sudden bursts of rapid fluctuations of temperature, usually of 0·1°F to 0·2°F, lasting over 500 ft to 1,000 ft. It can be seen even more clearly at this level that there is a temperature difference between the two sides of a burst, or bubble, cut by the line of flight of the aircraft. It was noticed that there was again good agreement between the occurrence of a bubble and the position of a cloud directly above.

(d) 2,000 *ft*

This trace is similar to that at 1,500 ft in that there are sudden bursts of temperature fluctuations of the order of 0·1°F to 0·2°F.

(e) 2,500 *ft*

The frequency of occurrence of the temperature bursts has decreased, and the start of the trace shows a slower temperature variation. (The sudden discontinuity seen after one minute of the trace was caused by an instrument change, and is not a real temperature change).

(f) 3,000 *ft*

On this run, the aircraft was always between 50 ft and 100 ft below cloud base.

The change between this and the traces at the lower levels is at once evident. There is a long wavelength fluctuation of ambient-air temperature; a cold pulse, nearly 12,000 ft across, is shown at the start of the trace, and several outbursts of rapid temperature fluctuation. There is also one well-defined pulse of warm air of about 0·3°F in magnitude. The total variation of

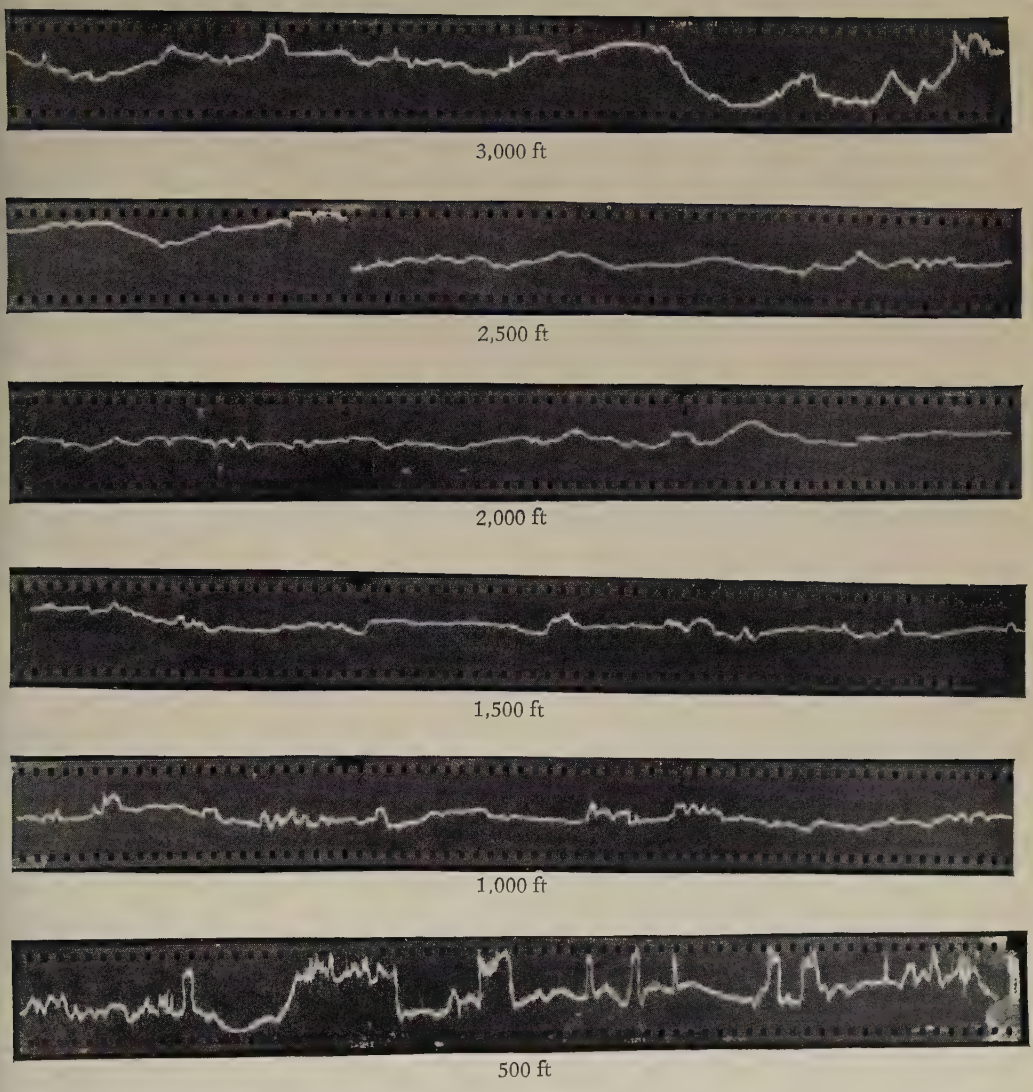

Figure 1. Fluctuations of temperature below cumulus cloud (in all the above traces the scales are horizontal length, 12,000 ft; vertical length, 1·2°F, full scale.)

temperature is greater than 1°F, and most of it occurs in the latter part of the trace. It was observed that there was no cloud directly above the aircraft at this point, but that there were clouds on either side of the line of flight. It is therefore possible that this large cold pulse is due to a downdraught of cold air produced by the development of these clouds. The smaller outbursts seen are considerably less in horizontal extent than those previously encountered, and may be due to the occurrence of local condensation and evaporation owing to the large amount of mixing at this level.

5. Discussion

The general picture given by these observations, (one flight only), of the mechanism of convection below cumulus clouds, under the stated synoptic conditions, is as follows.

The ultra-rapid thermometer traces may be divided into three groups. The first, at 500 ft; the second, of all the traces from 1,000 ft to 2,500 ft inclusive; and the third at 3,000 ft.

The observations at 500 ft show that in the lower layers there is a region of intense turbulent mixing, in which there exist bubbles of warm moist air of all sizes. These bubbles are well defined and seem to have no preferred space dimension. Few of them escape from this layer because of the intensity of the turbulent forces and mixing existing there. The direction of motion of the bubbles in this layer is determined by these turbulent forces rather than the buoyancy of such bubbles, as indeed is the length of life of any individual recognizable bubble. Obviously, those with the greater buoyancy have a greater chance of escaping, but this by no means rules out the possibility that some of the smaller bubbles also break away and rise towards cloud base. However, it is probably the larger bubbles which emerge from this layer which are responsible for the greater part of cloud development. On this flight, all that can be said concerning the thickness of this layer is that it was between 500 ft and 1,000 ft deep. It is likely that its depth varies from day to day and is a function of the synoptic situation, of the time of day, and of the topography of the ground under this layer.

The traces between 1,000 ft and 2,500 ft indicate a large decrease in the frequency of occurrence of the bubbles, and in their space dimensions. It seems likely that the largest bubble observed between these heights, which started from the ground, was at 1,000 ft, and was some 1,500 ft across. The smaller bubbles observed are well defined, but are too small and too numerous for them all to have started from the ground, in the light of the above argument. It therefore seems likely that most of these bubbles are part of the wake of the main bubble, having at one time been part of this bubble. It is possible that eddies, induced by the downward entrainment of air by the ascending parent bubble, give these small bubbles a component of spin or circulation about a horizontal axis. This could explain the small temperature difference always observed between the sides of such bubbles.

In the sub-cloud layer at 3,000 ft, there exist long wavelength fluctuations of temperature of 0·6°F to 0·8°F, accompanied by very short-lived fluctuations of 0·1°F to 0·2°F. It is possible that as a bubble condenses out, the colder air around the cloud is forced down towards cloud base by the development of the cloud. Reaching this level, the colder air is drawn under the cloud by the ascent of further air into it, and mixes with the warmer air already present at this level. Thus in the sub-cloud layer, we have the following :

(a) Warm moist air from the ground in the form of well-defined bubbles.
(b) Colder air which has been forced down from higher levels by the development of the cloud.
(c) More stable air, which was originally at this level, and which has been warmed by the passage through it of small and large bubbles.
(d) Small bubbles which were rising to the side of the main bubble, but which were forced down again by the descending air.

It is to be expected, therefore, that conditions of intense turbulent mixing exist in this layer.

It has already been implied that the very small rapid bursts of temperature variation seen on this trace are possibly due to local condensation and evaporation inside the bubbles. However, from the above, it seems equally likely that this phenomenon can occur simply by the mixing of the four types of air which are present at this level.

It may be observed that, up to the sub-cloud layer, the horizontal temperature variation of the ambient air decreases with an increase in height. This may be explained if we assume that any larger bubbles rising from the ground continuously waste away from their outer skins by mixing with the ambient air. Thus, as we get higher, the size of such bubbles decreases, and they therefore cause less disturbance to, and less temperature variation in, the air through which they pass.

Even under the existing synoptic conditions, it is extremely difficult to explain the significance of the association of the occurrence of bubbles with the presence of cloud observed above the aircraft. It may be that the path of one bubble from ground to cloud is a preferred path for future bubbles, as has been suggested by Scorer and Ludlam (1953), though this is no full explanation. Similar phenomena have been reported on other flights when the wind gradient and vertical shear were considerably greater.

It seems probable that the development of smaller clouds in the neighbourhood is assisted by the smaller bubbles which appear in the wake. This would mean that associated with the development of a large cumulus cloud, there will be slight development of several smaller clouds, by the pieces of the main bubble left in the wake. This may not always be the case, however, since it is likely that the majority of the smaller bubbles do follow more or less in the path of the larger bubbles. Alternatively, they may be caught and carried upwards by another larger bubble rising from the ground, or destroyed by mixing with the ambient air. Even if these smaller bubbles do not suffer any of these fates, it is not conceivable that they could start and maintain cumulus clouds on their own, since this would imply a continuous train of bubbles following the same path. The very nature of their formation precludes this possibility.

From the observations made, we can only guess at the shape of the bubbles recorded, but it seems obvious that the larger ones are certainly not spheres. In all probability their horizontal dimensions considerably exceed their height. Malkus (1952) suggests a preferred ratio of diameter to height of 2·5 to 3·5, though other ratios are possible.

6. Conclusions

It must be pointed out that although a description of only one flight is given here, several similar flights have been made. These conclusions are tentative in the extreme, and in all probability subject to amendment if not complete contradiction.

The conclusions reached from this flight are :

(a) Bubbles of warm moist air of sizes from 200 ft to 8,000 ft across form in the friction layer, and some of them ascend, preserving their identities as far as the sub-cloud layer.

(b) Between 1,000 ft and 2,500 ft, the frequency of occurrence of the smaller bubbles is much greater than that of the larger bubbles.

(c) The small bubbles encountered above 500 ft and below the sub-cloud layer are composed of air about 0·2°F to 0·3°F warmer than the ambient-air temperature.

(d) It is possible that the smaller bubbles possess a component of spin or a circulation about a horizontal axis.

(e) In the sub-cloud layer, the presence of different air masses produces considerable turbulent mixing, which results in large, long wavelength fluctuations of temperature. It is possible that local condensation and evaporation takes place at this level, and accounts for the very short-lived rapid fluctuations of temperature also observed.

Finally, I would like to list a few of the major points of the mechanism of convection, on which, I hope, some further observational work will be made.

(1) The exact size and shape of the larger convection bubble, and the temperature difference between it and the ambient air temperature at each level.

(2) A detailed analysis of the sub-cloud layer and an explanation of the phenomena observed there.

(3) The influence of orographic features on the characteristics of the convection bubble, and in particular the composition of a bubble formed over the sea.

(4) The significance of the positions of the small and large bubbles at all levels with those of the clouds observed above.

(5) Whether such bubbles do, in fact, possess a component of spin or circulation about the horizontal.

Acknowledgment

I should like to thank the Director of the Meteorological Office for permission to publish this paper.

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Thermals as seen by glider pilots

By. L. WELCH
Surrey Gliding Club, Lasham Aerodrome, Hants.

Although thermals have been used for the past 25 years it is surprising how little is known about them. The general description of a thermal which has been given by Mr. A. H. Yates is excellent and is one with which few glider pilots will disagree appreciably. It cannot be too strongly emphasized that there are wide variations in the types of thermals, in their strength, their size, their position and their general behaviour.

It is generally considered that convection gets stronger during the morning and then gradually dies away during the evening but this does not always appear to be so in fact. In the morning the thermals are often strong but quite small, while the last thermals of the day may still be strong although they are larger and further apart. That this is so, can be seen from a study of the barograph charts taken from gliding cross-country flights. The average slope of the curve showing the rate of climb seems to be remarkably steady right up to the last thermal used in the flight.

For any understanding of these thermal currents it is necessary to know what they feel like and so a description may be valuable.

If one is flying around on a day when there is convection, the area outside the thermal will never be completely calm. If one glides down in circles and a thermal comes up from the ground underneath, the glider will sink down into the top of it. Just before the actual boundary of the thermal is met, the rate of sink of the glider will decrease and then, quite suddenly, one will find oneself inside. It may seem surprising, but it is a fact, that the feel of the air inside the thermal is quite different from that outside. It is difficult to describe but the turbulence is of a different scale and the air has a kind of tingling feeling. Apart from this small-scale turbulence, the air is not smooth and surges up and down are met as one flies around. When the thermal is strong, these sudden surges can be quite violent. Normally, the edge of a thermal is fairly sharp and the entry and exit can be felt as a bump up or down. There are however, occasions when this general description does not hold good when the thermal itself may be remarkably smooth with very gradual edges. This state seems to occur on days when the thermals are exceptionally large.

When there is an appreciable wind blowing, there is a definite tendency for thermals to lie in lines more or less up and down wind. There may be little sink between the individual thermals in the same line but considerable down currents between the various lines of thermals. When wishing to travel at an angle to the wind, it often pays the glider pilot to go along a line of thermals and then fly straight across to the nearest likely looking place on the next line.

Sometimes, when climbing up in thermals, the entry into cloud is extremely easy. One just goes on circling and then later finds oneself inside the cloud. At other times, when one is near cloudbase, the thermal seems to peter out and it is difficult to find any current to take one up into the cloud. A long search may be required to find any lift and, when it is found, its area is often remarkably small. Even on the easy days, the area of lift under the cloud is usually comparatively small. It normally lies on the upwind side. Active cumulus clouds are usually considered to have flat bottoms. This is not always so. If the cloud has any vertical development the area of lift can sometimes be located by the fact that there is a hollow in the cloud at the point where it is being fed by air from underneath. This hollow may be as much as 200 ft high in the case of a big cumulus.

When soaring, a glider pilot, for obvious reasons, likes to keep as high as possible and therefore he is usually operating nearer cloudbase than he is to the earth. Cumulus cloud forms a reliable signpost of thermals and with experience it is possible to predict, with a high degree of accuracy, where lift can be found. However, when more than about 3,000 ft below cloudbase it becomes much more difficult; the type of cloud will show which areas are good but it is difficult to locate an individual thermal and so one has to consider the features on the ground.

It definitely appears that hilly ground is, in general, a very good source of thermals and also that an area with strong contrasts is more likely to be useful than a uniformly good surface. Individual pilots have their own theories and methods of locating thermals by reference to the ground but since these differ so much and often are contradictory, it is doubtful how much real knowledge they possess or how much of their success is due to chance.

It seems a great pity that the information which glider pilots have obtained about the atmosphere is not more generally known; this is a largely because they have written little in an accurate and analytical way, and that very few measurements have been taken. The reason for this seems to lie in the fact that, with few exceptions, the glider pilot and the meteorologist talk a different language.

This is not surprising since the outlook of the meteorologist is largely theoretical and that of the glider pilot is more empirical. The meteorologist still speaks about a convection current and refuses to call the thing a thermal, despite the fact that the people who actually use them have been doing so for the past 20 years, while on the other hand, the glider pilot indulges in vague theories which could almost be called witchcraft. Where co-operation between meteorologists and gliding people has existed both sides have benefited greatly.

In order to increase our knowledge of convection, it would seem important to realize that the conditions can vary to a very marked extent so that the conclusions drawn from a small number of observations cannot be applied to other conditions. Observations taken over a very large area would be exceedingly useful. For example – flights made simultaneously by a number of aeroplanes at various levels counting the number of thermals which they met would enable one to plot the thermal distribution over a particular area at a particular time – photographs taken at frequent intervals from a great height would enable the growth of cumulus cloud to be traced over a large area.

Another thing which would be likely to bring useful results would be the study of a comparatively small volume of sky for a short time from every conceivable point of view, by having aeroplanes and gliders flying through it, by studing the local wind direction on the ground by means of numbers of windsocks and smoke, by measuring the temperature at a number of points both on the ground and in the air, by releasing large numbers of ballons and observing the behaviour of the wind at frequent intervals. If all these observations could be taken for, say, two to three hours by a large number of people over an area 2 mi square, much could be learnt of the manner in which thermals leave the ground.

Gliders can be used as observation platforms for research into the structure of the air – of convection, of waves and of gusts. For some purposes they are much more suitable than aeroplanes but the amount of such work being done at present is very small. It seems unfortunate that more use is not made of gliders since the cost is low. Quite apart from this organized research more use could be made of the knowledge gained in ordinary flying if there were better co-operation. Finally, what real understanding can any meteorologist have of convection unless he has actually been up in a thermal even if only on one occasion.

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The sub-cloud layer over land

By F. H. LUDLAM

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Bunker *et al.* (1949) made detailed aircraft soundings amongst the trade-wind cumulus of the Caribbean. They found that below the cloud base the lapse rate was approximately dry-adiabatic, except in an uppermost layer several hundred feet deep, in which it was considerably less. They called this the ' sub-cloud layer,' and interpreted it as evidence that the updraughts in the clouds did not extend down into the adiabatic layer. In the sub-cloud layer there was also a considerable lapse in specific humidity, which was almost constant in the adiabatic layer.

The sub-cloud layer was most pronounced in large, almost clear areas (up to about 30 mi across) which separated clusters of cloud ; in the clusters it was hard to find. This suggests that it is caused by subsidence into the layer below the cloud-base level of air having the greater potential temperature and dryness characteristic of the cloud environment. In the air entering the clouds there may be an adiabatic lapse rate, but because, even in the cloud clusters, the updraughts occupy only a fraction of the whole area, an aircraft with a thermometer of ordinary response may still there be able to detect a sub-cloud layer.

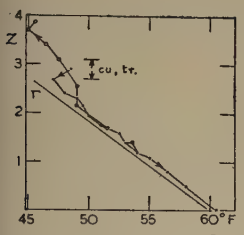

Figure 1. Sounding made at 15 hr, 16 April 1952 at Lasham, Surrey. The height z is shown in thousands of feet. The straight line shows the dry-adiabatic lapse rate.

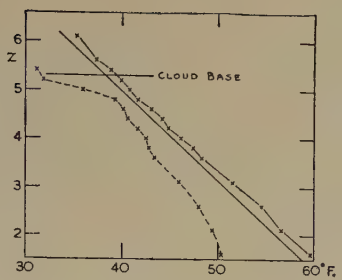

Figure 2. Sounding made at 15 hr, 14 August 1952, at Cranfield, Bedfordshire (dry- and wet-bulb temperatures).

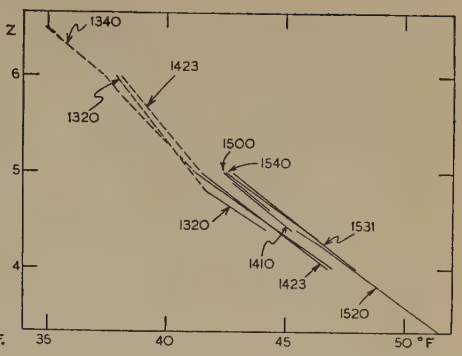

Figure 3. Glider soundings, 14 August 1952 at Cranfield. The pecked lines show soundings made inside clouds. The times at which each ascent began are indicated. The mean of the lapse rates below clouds is exactly the dry adiabatic.

This view is supported by soundings made during our recent investigations of cumulus over land. Fig. 1 shows a sounding made in a glider (towed aloft) at Lasham, Surrey, on an afternoon when there were only traces of small cumulus in air which was probably subsiding slowly. The dry-adiabatic lapse rate observed during the ascent ends a few hundred feet below the level of the cloud base, but a few thermals could be found in the adiabatic layer and it can hardly be doubted that the clouds formed in air which had risen from near the ground. In our work at Cranfield, Bedfordshire, we found that amongst well-distributed cumulus a change of lapse rate was sometimes noticeable a few hundred feet below the level of the cloud bases, accompanied by a more striking change in the lapse of wet-bulb temperature. Fig. 2 shows an example from soundings made by an Anson aircraft of the Cambridge University Air Squadron on 14 August 1953. On the same afternoon a glider several times ascended to cloud base in thermals, and during these ascents found dry-adiabatic lapse rates. Several such ascents are plotted in Fig. 3, which also shows that the potential temperature and the height of the cloud base increased slowly as the ground temperature rose. Humidity measurements were not made, but clearly the air temperature in the updraughts must steadily have approached the dew point as the air rose toward the cloud bases.

Acknowledgment

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Discussion

Mr. G. O. Smith : I want to make it quite clear, straight away, that I am speaking tonight essentially as a practical man. I claim no extensive scientific knowledge of meteorology, although I do hope that my general scientific knowledge is sufficient to prevent me from being too easily 'blinded by science' into believing the ridiculous.

Now it occurs to me that in speaking as a practical man about thermal currents in hilly country, at least 90 per cent of my remarks could sensibly be preceded by the words : 'My experience tells me . . . . . ', and this being so, it is only fair to overrule my natural modesty and tell you that my log-book and I reckon that I have done something over 400 hr soaring in

hilly districts between the months of April and September, i.e., when thermal currents may reasonably be expected. And from this experience, my conception of a thermal is that it is a bubble of rising air; certainly however not a spherical bubble, but rather a very long and sometimes very thin, sausage-shaped bubble.

Sometimes these bubbles are so elongated, and recur so quickly one after another, that one may be tempted to regard them as a continuous flow, in which case they would perhaps better be described as ' tubes ' than as ' bubbles.' However, I believe that this continuous flow is very rarely, if ever, the case, and I prefer to imagine them as sausages.

They are, of course, flexible and can be bent or otherwise misshaped by turbulence of the general air flow. As a sweeping generalization, I believe that they are more apt to be misshapen by such turbulence in hilly country than over flat country, and increasingly so with increasing wind strength; and I further believe that it is therefore a little more difficult to take full advantage of a thermal, i.e., to ' centre ' it and to remain in its centre, over hilly than over flat country. I am referring here, of course, to the lower part of a thermal, say within 2,000 ft of the ground. The higher you get, the easier it is, in either case.

Now, where to look for thermals ? I think it is reasonable to assume that they generally start from specific areas of ground, and furthermore that some areas are inherently more suitable for setting them off than others; and it has been argued that one ought therefore with experience to be able to mark on the map the exact areas in which to search for thermals. I do not subscribe to this view myself, because even if they did always start from the same known places (which I doubt), they would go off at different slopes and directions from day to day in accordance with the vagaries of the wind. And quite apart from the ' direction ' variable, I personally have always found the ' slope ' of a thermal an extraordinarily difficult thing to judge.

I agree that if on *a* day, you find a thermal over a certain gully in the hill-side, say, then that is a good place to go and look again. The *next* day however, which may be ostensibly similar, you may find that gully quite dead while consistent thermals are to be found over another place altogether.

I used to think that while hill-soaring and at the same time searching for thermals it is best to keep as far out in front of the hill as the hill-soaring current allows, in order to have the longest possible time in any thermal before deciding whether to continue in it or to return to the hill; but lately I have discarded this procedure in favour of keeping well back over or even behind the brow of the hill, as it seems to me that far more thermals come creeping up the face of the hill than rise independently in front of it.

Now, although I certainly do have this as a *general* rule in my mind, I am really talking here of my own particular hill in the north of Derbyshire, and the issue may possibly be confused by the local geology. Our hill consists of a large slab of gritstone resting on top of limestone, i.e., the country stretching out in front of the hill is limestone, while the hill itself and the comparatively flat plateau behind it are gritstone.

This brings up the much wider question of the best general districts in which to expect thermals, and while there is certainly not time here to delve into the respective specific heats, etc., of limestone and gritstone soils, I do feel that such a study would be worth while.

Thinking now on a scale covering a radius of say 10 mi, when the hill itself, as such, becomes almost negligible; I am convinced it is a fact that more thermals rise from the gritstone area behind our hill than from the limestone area in front of it; and I cannot therefore escape the conclusion that it is the difference in soil composition that causes this.

That is not to say, of course, that no thermals rise from limestone, or from our particular limestone area. We have a large cement factory, set in the heart of the limestone, which sometimes, though by no means always, actually produces excellent thermals; and, of course, the limestone area does produce its quota if conditions are good enough. The point is that its quota is a smaller one than that of the gritstone area.

I would like to close with a word of advice to prospective thermal hunters: ' never let yourself think you have got it all weighed up, because you haven't.' If you think about these things at all, you must of course formulate in your mind some tentative rules for the behaviour of thermals and air flow in general; but however right these general rules may be, combination of circumstances will undoubtedly disprove them from time to time. It is a bad thing, therefore, to get *set* ideas on soaring meteorology, because if you do, though you will probably find a thermal in some impossible place, what is much more important, you will most certainly fail to find one where one ought to be.

Prof. P. A. Sheppard : It is presumably no accident that the theoretical meteorologist has succeeded in giving us worthwhile treatments of the large-scale motions of the atmosphere

but has hardly begun to formulate a theory of cumulus convection. I would make a plea that he should turn his attention to this problem, whose difficulties are matched by its importance. Of the latter there can be no doubt, in the vertical transfer of heat, water vapour and momentum and the consequent reaction of the smaller on the larger scale, as I believe I illustrated at the last meeting of the Royal Meteorological Society. I need hardly elaborate the difficulties of dealing with systems which are themselves evidently quite closely related with smaller-scale turbulence, for which again no theory exists. But the moral of this meeting is that with the observations of gliders and aircraft the theoretician may be led to formulate a model which is realistic and which may in turn be more or less tested in the consequences by the same agents.

In formulating the model it may be desirable to warn against the danger associated with terms like bubble, cell and others heard this evening. Useful as they may be in free discussion none is yet defined in the way required by the mathematical physicist and one must be ready to find reality best represented by some entity sharing attributes with all. Success to the gliders – particularly in leading meteorologists to the model which ' works.'

Lt. Cdr. A. G. Goodhart : I must admit to being chiefly a practical glider pilot who is preoccupied with staying up rather than with the collection of data for the Royal Meteorological Society. Unfortunately, or perhaps fortunately, we do not fly around with five observers all gazing at instruments as do the gentlemen of the Farnborough Meteorological Flight. Accordingly, if we are to produce quantities of readings during our flights, I suggest that we need suitable recording instruments which will do the job for use while we stay up.

Now a few random thoughts. First, the relaxing pastime of watching the smoke rising from a bonfire. This may teach us some of the features of the normally invisible thermal. Apparently, although the whole thing is going up, the outside is on its way down relative to the core; with the result that when one enters the side of a thermal one first encounters rather weak turbulent lift before finding, if one ever does, the fast-rising core.

I also suggest that soaring birds can teach us a lot about thermals. I have found that they can detect a shift in position of the best rising air far quicker than I can, and I would very much like to know how they do it. It appears that, within about 1,000 ft of the ground, the best lift sometimes shifts quite suddenly by a few hundred yards and is of short duration, say 2 min, in each place.

With regard to catching thermals in front of a hill, my experience at the French National Gliding Centre at the foot of the Alps confirms Mr. G. O. Smith's remarks that they are better caught close to or even behind the crest of the hill rather than way out over the valley. The French suggest that the initial urge, imparted to the potential thermal by its being carried up the face of the hill, triggers if off; they call this type of lift ' thermodynamique ' which, if not technically accurate, is at least expressive.

My reasoning about the lack of lift frequently found beneath shallow cumulus is that because of a reduced temperature difference between the thermal and its surrounding air, it is rising more slowly, and although it goes on up to form cloud, it is not rising fast enough to overcome the normal sinking speed of the glider.

Finally, may I ask a question; do thermals rotate and, if so, is the rotation of any importance to the glider pilot ?

Mr. F. G. Irving : In August 1952, the Departments of Meteorology and Aeronautics, Imperial College, carried out some explorations of individual thermal currents by means of a Slingsby T.21b sailplane. This is normally a side-by-side two-seater, but for this purpose, one seat was replaced by an automatic observer and its batteries. The sailplane was towed up to a suitable height and then carried out traverses across suitable thermals, gaining further height in them if need be, whilst the automatic observer recorded rate of climb and sink, static pressure, air temperature and various other quantities.

The few films so far analysed confirm, in general, that the estimates of size, vertical velocity and temperature excess quoted by Mr. A. H. Yates are of the right order. The measured temperature excess happens to agree quite closely, but the actual figure is so small as to be only slightly greater than the possible instrument error. Further investigations will plainly require a very sensitive thermometer.

The conditions which Dr. D. G. James hypothesized as a result of the Meteorological Research Flight's findings seem to correspond with those sometimes prevailing before convection is sufficiently established for sailplanes to soar satisfactorily. This remark is based only on qualitative observation, but it may well be that Dr. James and the sailplane pilots have been describing related, but somewhat different phenomena. It therefore seems that, to gain further insight into the mechanisms of convection, both types of experiment should be pursued, *viz.*,

large-scale ' statistical ' data by the Meteorological Research Flight, and detailed investigation of individual thermals by sailplanes.

Wg. Cdr. R. M. Poulter : There are a number of interesting points to discuss, but as the hour is late I will only refer to the sub-cloud layer dealt with by Dr. D. G. James and Mr. F. H. Ludlam. When rising air reaches the condensation level, cloud is not immediately visible although the lapse rate changes from the dry to the wet adiabatic (see Fig. 1). On many mornings humidity is 100 per cent without visible mist or fog. When ' thermals ' just exceed the condensation level, one can often see bluish mist patches which do not become cloud. A supersaturation of about 0·3 g/m^3 is required to form a small white cumulus cloud and this requires a further rise of about 250 ft, thus explaining the sub-cloud layer.

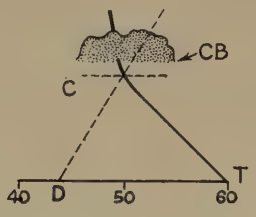


Figure 1. C = condensation level, CB = cloud base, T = dry-bulb temperature, D = dew point.

As the water drops spread to the outside of the cloud and descend one can often see, when the sun is in the right position, delicate pink and green iridescence before the drops evaporate. I have spent many hours on holiday watching the process through dark glasses. Sometimes drops are caught in an updraught and appear white again.

Mr. J. Pothecary : The impression that measurements by means of the ultra-rapid thermometer and hot-wire anemometers, used by the Meteorological Research Flight in the Hastings aircraft, are not comparable with measurements made from sailplanes is not founded on fact.

The lag of the ultra-rapid thermometer is about 5 millisec and of the hot-wire anemometer, about 20 millisec. The lag of the equipment used in the sailplane in the Cranfield investigations is not stated but it is generally assumed that for thermocouple measurements of temperature the lag is of the order of 0·5 sec. The speed of the Hastings is about three times the speed of a sailplane.

An important feature of the Hastings work is that a reasonably accurate overall picture of conditions at any one time is available. This is not possible by the use of sailplanes alone.

Capt. F. Foster : I should like to make the following points :

The internal construction of convection thermals differs widely from day to day when noted at the same hour. Wind shear and turbulence can only account for these differences in the lower layers. Over a period, I have become convinced that the internal structure, size and frequency are connected with moisture content to an appreciable degree. These factors I should like to see measured in relation to other factors of lapse rate, barometric tendency, depth of unstable air and surface temperature. Other things being equal, thermal activity is smoother on days when humidity is high; days with small amounts of cloud are often rough. The up current, especially of the more powerful central core, is of smaller diameter on days of low humidity.

I should like to endorse Mr. Yates's remarks that, could suitable instruments be made available, there are many people in the gliding movement who would be interested to assist in measurement of convection structure.

Mr. O. W. Neumark (*partly communicated*) : I would like to refer to Mr. Welch's statement that there is no single standard type of thermal and ask him whether he has ever met a thermal with ' heart shaped ' isopleths ? They are rare but do exist. My attention was first drawn to them by the late Mr. D. F. Greig and I subsequently encountered them on several occasions. The wedge of sink, or reduced lift, encountered on the downwind side, is met when one has centred correctly in the area of highest lift which is always very strong and narrow. If one makes a model of a series of annular vortices puffing up through each other, and tilts the column to simulate the effect of a marked increase of wind with height, the horizontal cross-section will give ' heart-shaped ' isopleths.

Mr. Welch was the only speaker to refer to ' feeling ' turbulence which is not so unscientific

as some might infer. Turbulence produces noise, especially when a sailplane passes through it, and this is further amplified by the excellent ' sound-box ' qualities of the sailplane structure. Can Mr. Welch confirm that his definition of ' feeling ' includes the sense of hearing as well as the sensing of accelerations ?

In the study of small-scale convection or ' thermals,' slow and relatively noiseless sailplanes have certain obvious advantages over high-speed Hastings of the Meteorological Research Flight. Free balloons, however, provide even better observation platforms because they do not produce noise themselves. Noise is a reflection of the degree of physical disturbance produced in the atmosphere and if it is very great, as in the case of the Hastings or even appreciable as in the case of a sailplane, the mere presence of such coarse observational probes might metamorphose the phenomenon which is to be studied.

I would like to emphasize the very important point made by Mr. L. Welch and in part by Mr. A. H. Yates. Investigation should proceed along two lines : (a) concentrating all our scientific resources to investigate *one* individual thermal in contrast to the Hastings investigations which are spread over a considerable air-space and time, (b) obtaining information from as many thermals as possible by loaning self-recording instruments to many sailplanes.

In regard to the first line of approach, the value of free man-carrying balloons has already been referred to but, although visible thermals from heath fires were mentioned, the obvious step of making a thermal visible by shooting lattice volleys of smoke-producing rockets on horizontal and vertical flight paths through thermals was not emphasized.

Smoke canisters ignited from sailplanes have been used in the past, but they are too expensive. Here the Royal Meteorological Society might use its influence, through the Meteorological Office and Air Ministry, to make available to sailplane pilots and clubs R.A.F. smoke canisters, smoke-puff Verey cartridges and signal rockets, which are destroyed in great quanties by armament inspectors, because the number of missfires in a given batch is too great for Service requirements. This would not disqualify them for use in thermal research. The use of ' condemned ' pyrotechnic stores in conjunction with sailplanes and photography would immeasurably advance our understanding of thermals.

In respect of the second line of approach, I would like to recommend sailplane pilots to mount their cameras ready for use on a universal joint and clamp mounting as so very many valuable cloud photographs are missed because one's camera is in its case and not ready for immediate use. Leading aerial camera manufactures might be interested in furthering meteorological research by loaning cameras and film material for permanent installation in high-performance sailplanes.

Regarding self-recording instruments for use in sailplanes, it is not a very difficult task for a well-equipped instrument workshop to produce thirty to fifty meteorological flight recorders, utilizing radiosonde sensing elements recording on smoked charts. Alternatively, sailplane pilots might have humidity and temperature elements (from radiosondes) added to their barographs. This could be done with a Fuess barograph although the three traces would be displaced on the time scale.

The loan of miniature portable wire or tape sound recorders, as used by the B.B.C. or test pilots, would be very welcome indeed and might reveal new noises. They can also be used for determining the effect of various aerodynamic improvements to the sailplane in terms of ' noise.'

Regarding Mr. Welch's point about contrast as an all-important factor in the production of thermals, he might recollect that free balloonists for the past hundred years have often remarked on the turbulence and noise encountered when crossing rivers. They often stated that their balloons started to spin, which might shed considerable light on rotation in thermals.

I, and no doubt many others, have for many years counted the number of birds circling to the left or right. Usually the distribution is random although on a very few occasions I have detected a marked preference for right-handed circling (in the northern hemisphere).

I am very glad that Mr. Welch mentioned that he wished there were more high-level photographs of cloud. High-level photographs from 60,000 ft-100,000 ft from balloons, from 100 km-385 km (the highest yet) from rockets and, eventually, continuous television from orbital stations will revolutionize our meteorological services. In 1947, Dr. A. E. Slater and I attempted to correlate V2 photographs with synoptic data ; one isolated paper on this subject was produced in the U.S.A. by D. L. Crowson (*Bull. Amer. Met. Sec.*, **30**, 1949), but no official interest has been evoked by the untold possibilities of really high-level photography. The effect of topography on cloud, both frontal and anticyclonic, the curious 'swell' patterns and other phenomena can only be detected from an elevated point of view. The proverb of not being able to see the wood for the trees is very appropiate in this context.

Lastly, I would like to comment on Wg. Cdr. Poulter's interesting observations. Since I first read his observations on iridescence many years ago, a pair of polaroid spectacles have enabled me, by cutting out swamping blue scattered light, to observe iridescence in clouds, whenever they were near the sun. Meteorological text-books treat iridescence as a comparatively rare phenomenon; however, when polaroids are used, iridescence can always be seen where cloud droplets are very small, usually where they are evaporating but also where they are forming.

Contrary to Wg. Cdr. Poulter's early observations, I have many times seen iridescence on the leading edge and top surface of lenticular clouds.

The sub-cloud layer often becomes visible with polaroids before clouds or even milkiness becomes visible to the human eye, even when using unpolarized dark or coloured glasses.

Although text-books on optics often state dogmatically that white reflected light from clouds is not polarized I have on many occasions detected marked polarization from certain clouds, and nearly always from haze levels marking inversions.

Has Wg. Cdr. Poulter used polaroid spectacles in his observations ?

Wg. Cdr. R. M. Poulter (*in reply, communicated*) : I agree with Mr. Neumark's remarks on the frequent occurrence of iridescence in clouds although I have not used polaroid spectacles in this connection.

Concerning his remarks about lenticular clouds, water drops blow through these clouds and the ' leading edge ' would be the region of evaporating drops, I think.

Dr. R. S. Scorer (*summing up*) : It is clear that on the subject of fair-weather convection everyone feels confident enough to express his own opinion. This shows not only that the subject is in its very elementary stages and every opinion is worth consideration but also that there is a great variety of phenomena and every observer has a very real, though not identical, experience on which to base his ideas. It is easy to find fault with a simple theory for it is equally possible that some of the motion observed has nothing to do with cumulus convection directly.

Together with the great complexity of the motion we are concerned with several methods of observation each capable of observing only one of the many aspects of the motion. The fast aircraft with delicate instruments can survey a large area, but can only have a fleeting glimpse of an individual thermal. The glider pilot, unless he is either more scientist than sportsman or else a very rich man, must usually make measurements in up currents most of the time, but he can explore a thermal. Unfortunately his experiences are not uniform enough to come within the scope of one simple theory, and are not numerous enough for statistical treatment. These various methods must not be regarded as rival but as complementary and each of us explain not only his own but the other fellow's observations too, for we are all observing the same thing.

In order to embrace all these methods, Mr. Ludham and I have developed a rather more sophisticated bubble theory. Although published before this meeting, it really comes after it, for we had the benefit of the ideas presented here, and it is high time that they appeared in meteorological literature. Most of us have probably had a pretty good idea of what a bubble is after hearing Mr. Yates; but it is not easy to describe satisfactorily its origin and where it goes to, and when we try to explain, in terms of it, all the experiences of pilots, without violating the fundamantal laws of meteorology and fluid motion, we are not quite so sure of its simplicity.

It seems that more reliable observations are required. Glider pilots should be encouraged to carry thermometers and reliable variometers and their reports should be more than informal talk. We ask for more written observations and, in turn, the meteorologist must treat pilots' accounts more seriously. Together with the meteorologist's scepticism of reports goes the pilot's faith in his own memory coupled with sweeping generalizations based on his own pet theory. This has only encouraged the disregard of all his reports which have tended to be explained away instead of explained.

Meanwhile we have great expectations for the Meteorological Research Flight, and hope that they will soon be able to make simultaneous observations of more variables with their new instruments. While we ask the glider pilots for more scientific reports, we also hope that red tape will not inhibit the quick sharing of the Meteorological Research Flight observations as well.

It is becoming increasingly clear that convection is a process which meteorologists will have to take into account more and more, for it is involved in the development and decay of almost every major weather system.

551.574.1 : 551.575.1 : 551.591.1

Condensation nuclei and the development of radiation fog

By A. C. BEST

(Read 18 February 1953. See *Q.J.*, **79**, p. 112)

PROF. P. A. SHEPPARD : Mr. Best's Table 1 shows that very long times are required for the larger nuclei to grow to equilibrium size when the humidity approaches saturation. In view of this, are the water contents given in Table 3 for larger nuclei likely to be any guide to the reality of a growing fog ? Is it not indeed true that the growth of fog droplets can only be realistically treated as a ' dynamical ' rather than a ' statical ' problem ?

MR. B. J. MASON : In view of the assumptions which Mr. Best has to make concerning the nature, concentration and size distribution of the nuclei (matters on which we have practically no information), and particularly because he considers only the early stages of condensation when the relative humidity is 100 per cent or less, I am not certain that his paper throws very much light on what we might expect to find in a well-developed fog. Does not the most important part of the story begin only when the relative humidity closely approaches and exceeds 100 per cent ? It is then that the largest contribution is made to the liquid-water content and opacity, so that measured values of water content W and drop size in a well-developed fog would probably bear little relation to Mr. Best's computed values which are based on the assumption that the drops grow in equilibrium with the prevailing humidity. As shown in his Table 1 the latter assumption is certainly not valid for the larger nuclei but it is just these which contribute most to the liquid-water content even under just saturated conditions.

The nature of the responsible nuclei is still an open question but I would suggest that the evidence is beginning to point against sea-salt particles being the dominant agents over the land. If one considers these to be the smaller hygroscopic nuclei of industrial origin, the assumption of equilibrium growth up to 100 per cent humidity has greater validity but the growth *rates* of these smaller nuclei will increase much more rapidly when supersaturation sets in than will be the case for the larger nuclei considered by Mr. Best; so, even in this case, calculations which stop at 100 per cent humidity are not comparable with measurements made in the dense fogs where the droplets are much larger than their equilbrium radii at just water saturation.

It appears to me that the growth of droplets in fog and clouds must be considered as a time-dependent process and if the larger nuclei are to play an important role this should be done even in the early stages considered by Mr. Best.

MR. J. S. SAWYER : When attempting to forecast the onset of fog at an airfield I noticed that, when the air was clean and the initial visibility good, the appearance of fog was usually sudden. On the other hand, when the air was already hazy before the fog, the deterioration of visibility was gradual. This difference in the process of fog formation between clean and polluted air may or may not have its cause in the micro-physics of condensation; other factors such as the radiational properties of the air may be important. However, my reading of Mr. Best's paper is that the explanation is not to be found in the differences between the size or number of the nuclei. I should like to hear Mr. Best's views.

DR. R. LESSING : May I, as a visitor and a chemist, refer to the influence on fog of the acid and smoke pollution of town air. The author may be correct in his contention that neglect of industrial nuclei will not have any serious effect on the causation of fog, although the greater hygroscopicity of sulphuric acid as compared with sodium chloride must influence the relative growth of nuclei.

Even if the acid were not primarily responsible for fog formation, it must exert a considerable stabilizing influence on its development. In the bad fog of 5-8 December 1952, the average daily emission of sulphur oxides into the air of Greater London was over 500 tons in terms of H_2SO_4. It was significant that the SO_2 content of the air rose steadily until it was more than doubled on the third day. From the practical point of view, it seems of little consequence whether the acid pollutant was the cause or the effect of that catastrophic fog incident which, apart from material damage and high morbidity, caused the death of 4,000 people.

Mr. E. Gold : It is the difference between the vapour pressure of the ambient air and the vapour pressure of the drop which determines the rate of condensation; and a change of relative humidity from 100 per cent to 100·3 per cent would mean so small an increase of vapour pressure of the ambient air that it could hardly produce a change in the time of growth, from 5,000 sec to 25 sec. As Prof. Sheppard remarks, this problem is a dynamic one and I think Mr. Best should be given time to see what he can make of it. The formula connecting visibility and extinction coefficient is due to Koschmieder. Possibly the reference to his paper could be quoted.

Mr. A. C. Best (*in reply*) : I think that the criticisms put forward by Professor Sheppard, Mr. Mason and Mr. Gold are really all based upon a single inadequacy of this paper, viz., that only the early stages of development of the fog are considered. I entirely agree that the development after the relative humidity has reached 100 per cent may be more interesting but there are difficulties in the theoretical treatment of a collection of nuclei of different sizes after the humidity reaches saturation, difficulties to which I have not yet obtained a satisfactory solution. It is for this reason that the paper does not purport to consider more than the early stages. Even for these early stages my main purpose was not so much to obtain absolute values for the liquid-water content as to see how this quantity varies with size and size distribution of nuclei.

Professor Sheppard's query relating to the slow rate of growth of the large nuclei in near-saturation conditions is well founded but I think he has also provided the answer in pointing out that the humidity does not remain constant in practice. I have recomputed the final entry in Table 1 on the assumption that the final humidity is 100·1 per cent. The result is to reduce the time from 5,200 sec to 560 sec. I think this also answers Mr. Gold's point.

With regard to Mr. Sawyer's remark I think the rate of covering must be greater when the air is clean than when it is noticeably polluted and assume this would cause a more rapid deterioration in visibility.

I am sorry I omitted to give Koschmieder the credit for Eq. 18. This equation is based upon Koschmieder's work published in *Beitr. Phys. frei Atmos.*, **12**, p. 33 and p. 171, 1924 and in *Mon. Weath. Rev.*, U.S.W.B. **58**, p. 439, 1930, though I believe it is necessary to refer to Middleton's book for the numerical value of the constant.

551.509.34 : 551.521.2 : 551.524.36

Some further aspects of night cooling under clear skies

By W. E. SAUNDERS

(Read 18 February 1953. See *Q.J.*, **78**, p. 603)

Dr. H. L. Penman : These comments are based on reading of the first three pages of the paper and have their starting point in Fig. 2. To me the dotted lines are merely the boundary between day and night, for an equally good fit to the observations is obtained by a smooth curve which passes through a point about 40 min after sunset in December and about 80 min after sunset in June and with graduated lags in between. Accepting this, what can cause the discontinuity ? There is no first-order meteorological discontinuity between light and dark, but there is a first-order biological discontinuity. The transpiration process takes place through stomata in the leaves and the stomata close in the dark, so there should be a fairly abrupt cessation of transpiration round about lighting-up time. As transpiration is a major component in the energy balance, I suggest that the kink in the cooling curve is not due to the sudden appearance of a new heat source – dew formation – but is due to the sudden disappearance of an old heat sink, transpiration.

Consider a small element of volume at the surface, with a heat content, Q, say. In effect we are studying the value of $-\frac{dQ}{dt}$. Before and after darkness the balance equations are :

before, $$-\frac{dQ}{dt} = R_B + E - (S + K)$$

after, $$-\frac{dQ}{dt} = R_B - (S + K)$$

where R_B = rate of long-wave back radiation,

E = rate of transpiration,

S = rate of upward flow of soil heat,

K = rate of downward flow of air heat.

For the Texas example of Fig. 1, we can work out the orders of magnitude of these quantities. They are in cal cm^{-2} hr^{-1} : $R_B \simeq 10$, $E \simeq 6$, $S \simeq 7$, $K \simeq 2$. The theoretical cooling rates, before and after dark, are thus 7 and 1 cal cm^{-2} hr^{-1} respectively. The observed rates at the surface are 7°C/hr and 1·1°C/hr respectively.

So far, so good : but the kink could occur earlier. Transpiration can be checked by decreasing the vapour-pressure gradient, so if the dew point is high then the leaf temperature may fall to dew point in daylight, transpiration will stop and the same phenomenon will occur. In this case however the transition will not be very sharp and the change in slope will be much less obvious.

Some of these ideas need more examination in detail, but as a corollary, if my explanation is correct the opposite effect should occur before sunrise. There is an indication of it in the Texas record and some of our own records show it fairly clearly – an increase in cooling rate at dawn.

Mr. B. J. Mason : The evening temperature discontinuity near the ground has been attributed to the onset of dew formation by Mr. Saunders and to the cessation of plant transpiration by Dr. Penman; it is not difficult to think of other effects which may be relevant. Measurements made over the desert or over extensive bare soil, should help to decide between these two explanations. Has, in fact, the temperature discontinuity been observed in the absence of vegetation ? Its existence under these conditions would decide against the transpiration theory.

Mr. J. L. Monteith : Mr. Saunders in his first paper on this topic suggested two mechanisms which might be responsible for the temperature discontinuity. The first was the release of latent heat by dew formation and this he has discussed tonight. The second was based on the observation by Durst that on inversion nights the wind gradient increases until the laminar flow becomes suddenly unstable and breaks down. Subsequent turbulent mixing brings warmer air to the surface.

I find the ' mixing ' hypothesis the more attractive of the two for the explanation it provides of at least two points where the ' dew ' hypothesis runs into difficulty.

Fig. 1 shows that the maximum discontinuity occurs at the lowest observation point above the ground. The discontinuity becomes less marked with height but has in fact been observed at 100 m (*Geophys. Mem. Met. Off.*, No. 89) This is the effect which mixing would be expected to produce since the greatest temperature changes would occur in the region of the steepest initial gradient. The effect takes place almost simultaneously at all levels. Again, this would be expected with mixing whereas an additional heat source at the ground would take an appreciable time to make its presence felt at, say, 100 m in very stable flow conditions.

The mixing effect can obviously produce a discontinuity on nights with no apparent dew formation. Mr. Saunders has suggested that on such nights condensation may take place invisibly on the soil surface but not visibly on grass, implying that the actual surface temperature may be lower than that of the plant cover. I should have thought this unlikely since the surface receives a considerable amount of heat by conduction from beneath while the tips of the blades are well insulated from this heat source. Perhaps Dr. Penman can offer some guidance here.

Dr. R. C. Sutcliffe : The theoretical points are of course interesting but we should not lose sight of the fact that Mr. Saunders is primarily a practical weather forecaster seeking a scientifically intelligent and practically useful way of handling a routine problem. He is, I think, very much to be congratulated in so successfully systematizing a method which is probably used in a rough qualitative way by many forecastors. The occurence of a rather sudden change

in cooling rate around dusk, when the temperature reaches a value depending mainly on the air-mass characteristics, was certainly recognized as of real practical importance in wartime operational forecasting of night visibilities.

We have heard no fewer than four theories this evening : deposit of dew, cessation of evaporation from vegetation, changes in radiation balance and the breakdown from smooth to turbulent air-flow. In this state of knowledge it is not surprising that the forecaster must use empirical methods.

Mr. E. Gold : I observe that the standard of time is not shown in Fig. 1, nor is the time of sunset indicated. If the time is local apparent time, then the discontinuity occurred nearly an hour after sunset. On a clear evening, however, dew begins to form on grass even before the time of sunset. The heat provided by deposition of dew from the air is, however, sufficient in amount to cause the slackening of the rate of cooling indicated in the paper. If the lowest metre of air deposited about 0·002 mm of dew, i.e., about one-fifth of its moisture content at 10°C, the latent heat released would warm the air 5°C or stop it cooling 5°C. If in Fig. 1 the cooling at 2 in. had proceeded for an hour, beyond the discontinuity, at its initial rate it would have brought the temperature 5°C below the value actually recorded. Dew at the rate of 0·002 mm/hr does not seem excessive at a time near sunset on a clear night.

I agree with Dr. Sutcliffe that this is an interesting and useful paper.

Mr. L. C. W. Bonacina : I feel doubtful whether the relationship between the screen and grass minima is so simple as Mr. Saunders's mean values suggest and should like to ask him to what extent in his experience anomalous cases occur. I may mention that at the Hampstead Observatory on the night of 6 Feb. 1952, when the screen minimum was 24·6°F the grass minimum was 12·0°F and on the following night the respective figures were 23·9°F and 18·5°F, evidently an exception to the rule. On the first evening at about 20 GMT I noticed that the neighbouring pond froze over very suddenly.

Mr. R. K. Pilsbury (*partly communicated*) : I should like to say a few words in support of Mr. Saunders's paper. Although Dr. Robinson states he cannot find the discontinuity in the cooling rate, it is plainly visible in some of the diagrams given in Dr. H. L. Penman's paper, ' Daily and seasonal changes in the surface temperature of fallow soil at Rothamsted ' (*Q.J.*, **69**, p. 7, 1943). In this paper he has indicated that in considering soil and air temperature, one may think of two seasons, summer and winter, with transition periods in October and April. In the summer the soil is warmer than the air, but in the winter both are at almost the same temperature. I have been investigating the variation in the time of the temperature discontinuity shown in Fig. 2 of Mr. Saunders's paper during 1948-1951 for Northolt.

I have compared the screen, grass minimum and one-foot earth thermometer for Southgate, where the soil is clay similar to that at Northolt. The sudden change in October of the time of the temperature discontinuity appears to take place on the evening following the first severe ground frost, sharp enough to cause a reduction of a degree in the one-foot earth thermometer. It is therefore suggested that this severe cooling is sufficient to reduce the soil temperature to the winter condition, mentioned by Dr. Penman, in which the soil surface is continuously at or near 100 per cent humidity.

Thus if the soil humidity, due to the excessive cooling mentioned above, has reached 100 per cent, the dew point of the air near the surface will be reached at an earlier hour in the evening and could thus account for the abrupt change in October.

Mr. W. E. Saunders (*in reply*) : It has been shown by Dr. Penman and Mr. Gold that the cessation of plant transpiration and the onset of dew are both capable of producing a discontinuity of the order observed. Other speakers have stressed that the breakdown in laminar flow, suggested in my 1949 paper as the cause of discontinuity, provides a better explanation of the known facts. Screen-level discontinuities of this type occur in Northern Nigeria at a time of year when the terrain is semi-desert, and Mr. D. Cruickshank has written that he has noted similar discontinuities at desert stations in Iraq. These facts, of course, support the turbulence argument. In this country the time of discontinuity appears distinctly less variable in the late summer and early autumn than in the spring, which seems to associate the time of discontinuity with the state of ground. The October change, as Mr. Pilsbury emphasizes, marks the point at which the ground becomes permanently wet and cold. These facts about the time of discontinuity stress the importance of the underlying surface. I think the full explanation of the discontinuity will require continuous observations of temperature, humidity and wind, from the ground to some 300 ft, together with observations of state of ground, and of any condensation or moisture release from the vegetation. This will be needed for various types of soil.

Replying to Mr. Gold, local time was used on Fig. 1. The time of discontinuity was therefore very similar to that found in this country in September. Regarding dew formation, my observation was that there was very slight moisture on the grass before sunset but there was very sudden and copious dew formation at the times mentioned after sunset.

I agree with Mr. Bonacina that the relationships probably are more complex than suggested in this paper, but reference to earlier proposed relations between screen and grass minima will show that we have at least gone some way towards allowing for more of the obviously significant variables. The relations were derived for the soil and exposure at Northolt. They do not necessarily apply elsewhere, though it should be possible to deduce a similar relation for another site. Some idea of the incidence of anomalous cases is given by the standard deviations quoted in Table I. The number of cases used in each section of Table II was small, and the accuracy has not yet been fully tested. The nights mentioned in Feb. 1952 appear from the *Daily Weather Report* to have been cloudy or partly cloudy in the London area. If this was also true of Hampstead, the occasions are of course outside the scope of this work.

551.574.1 : 551.578.41

The growth of ice crystals in a supercooled water cloud

By B. J. MASON

(Read 18 March 1953. See *Q.J.*, **79**, p. 104)

Dr. I. C. Browne : Mr. Mason has predicted that hexagonal plates of ice will show dendritic growth only if their diameter exceeds 100 μ by some unknown factor. During 1952 I was able, with the co-operation of the Cambridge University Air Squadron, to take plastic replicas of ice crystals in warm-frontal cloud which was giving a rainfall rate between 1 and 2 mm/hr. The samples were taken at heights corresponding to temperatures between -5°C and -15°C, and showed the coexistence of needles and hexagonal plates in the regions investigated. The presence of supercooled water droplets at all heights was indicated by the occurrence of light icing on the aircraft. The needles were up to 700 μ long, their breadth being roughly a quarter of their length. The hexagonal plates were up to 400 μ in diameter; none showed signs of dendritic growth. Thus the value of Mr. Mason's unknown factor appears to be at least four.

Mr. E. Gold : Would Mr. Mason explain why ρ is greater than ρ_s if T_s is greater than T ? One would expect a greater vapour density at the higher temperature.

Mr. B. J. Mason (*in reply*) : The observations made by Dr. Browne are very interesting and valuable; more information of this kind is badly needed. I have occasionally found hexagonal plate crystals of up to 700 μ in diameter showing no signs of sprouting at the corners. On other occasions dendritic growth appears when the diameter is only 300 μ. I think that additional factors which determine the size at which the transition occurs are the temperature and supersaturation, and in the case of a water cloud, the size and concentration of the droplets.

In reply to Mr. Gold, in my Eq. (1), ρ_s is the vapour density at the surface of the crystal (assumed to be nearly equal to the saturation-vapour pressure relative to *ice* at the surface temperature T_s of the crystal) and ρ the vapour density of the environment which is saturated with respect to *liquid water* at the temperature T of the environment. As T_s exceeds T by only a few tenths of a degree, ρ will exceed ρ_s.

551.501.81 : 551.574.1 : 551.576.11

Theory of the radar upper band

By R. WEXLER

(Presented by Mr. E. K. Bigg)

551.501.81 : 551.515.8 : 551.577.11

Precipitation streaks as a cause of radar upper bands

By I. C. BROWNE

(Read 18 March 1953. See *Q.J.*, **78**, Pp. 372 and 590)

MR. R. F. JONES : I should like to attempt to resolve the differences which are apparent between these two papers. Wexler accepts the existence of Bowen's horizontal upper bands but not his explanation, while Browne doubts not only Bowen's explanation but also the existence of such bands at all.

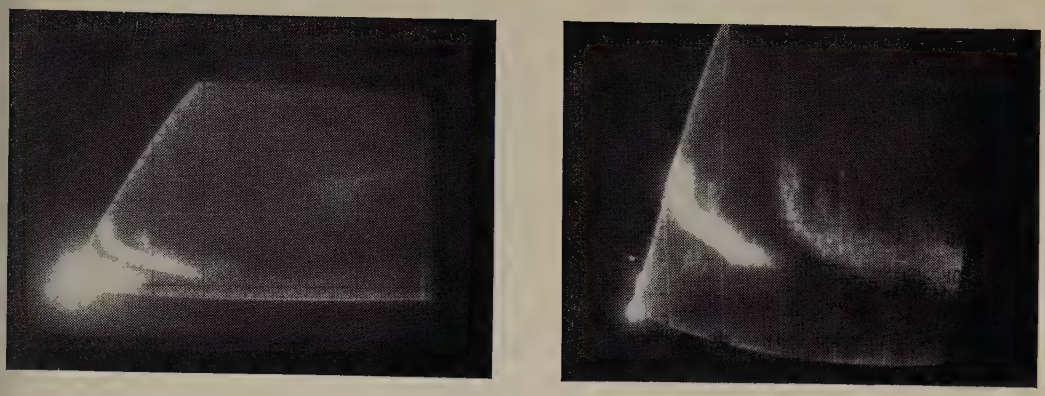
I have two photographs, taken at East Hill, of the display, using sweeping height-range radars, which demonstrate quite conclusively the reality of these precipitation streaks. The first example (Plate IV, Fig. 1 (a)) was taken using a 10-cm wavelength and the effect of wind shear is clearly seen. This photograph was taken on a bearing of 207°T and the wind at 15,000 ft was 205°T, 45 kt, so that, if there had been a ' vertically-looking ' set at East Hill, a falling echo maximum would have resulted as this streak moved overhead. It should be noted that this type of display exaggerates the slope since the vertical scale is about five times as wide as the horizontal. The streak here has a real slope of about 1 in 17 between 9,000 ft and 3,000 ft. Similar remarks apply to the second photograph (Fig. 1 (b)), taken on a 3-cm radar. Here you see a second streak following the first which would give the impression with a ' vertically-looking ' radar of a band re-forming, after an interval of time, at the original height – a phenomenon reported by Bowen in his paper.

So far the evidence supports Browne, but his falling speeds were always greater than Bowen's, and in fact one of Bowen's bands did not fall at all. This could be explained if Bowen was scanning perpendicular to the wind direction, but I have two more photographs here, chosen out of a larger number available, which I think show horizontal bands. The first photograph (Plate IV, Fig. 2 (a)) shows a band at 12,000 ft temperature − 4°C, extending horizontally for 45 mi with no melting band below. The second (Fig. 2 (b)) is not so clear an example but is included because it was investigated by the Meteorological Research Flight. In the upper band of echo at 10,000 ft to 11,000 ft the temperature was about − 7°C, the ice-crystal size and rate of precipitation recorded from the aircraft were not noticeably different from elsewhere in the cloud above the freezing level, but there was heavy icing compared with little icing elsewhere in the cloud. I think this is significant and suggests that the band is associated with the rapid growth of ice crystals in the presence of considerable supercooled water ; or even that the ice crystals are wet.

Frontal clouds occur quite frequently in layers rather than as solid cloud from Cs to Ns. If ice crystals fall from a Cs layer into a cloud layer of supercooled water droplets, they may grow large enough to give an echo. If the air beneath the cloud is dry, the ice crystals, on falling into it, will begin to evaporate and the echo will disappear and you will observe a stationary band of echo. The width of the band may slowly increase as the air beneath becomes more moist through the evaporation of the ice. If the ice crystals are so numerous that they clear the cloud layer of water, nearly completely, and the layer beneath is not too dry, you will see an echo band falling at about 1 to 2 m/sec. This I think, is similar to the explanation that Wexler gives except that I have assumed the ice crystals to be present through a ' seeding ' effect.

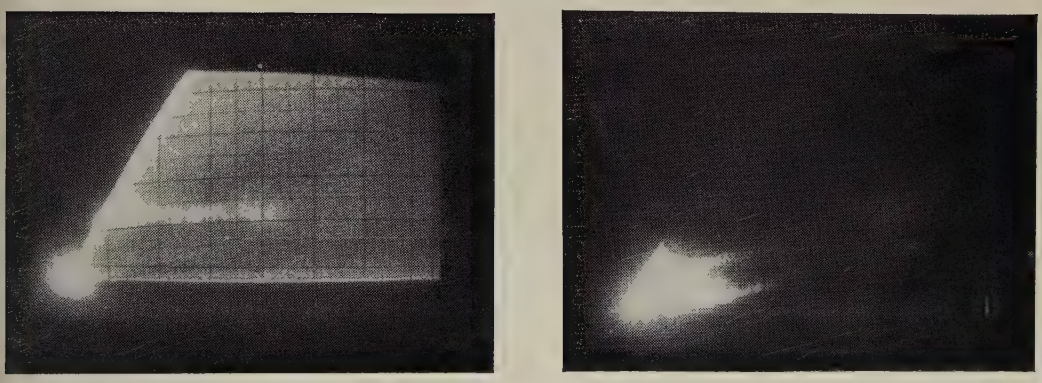
It looks then as if some of Bowen's bands were really horizontal bands and some were precipitation streaks and that both papers can be reconciled with each other.

MR. B. J. MASON : I have recently had the pleasure of discussing the interpretation of these ' upper ' bands with Drs. Bowen and Wexler and also with Dr. Marshall of McGill University.



(a) (b)

Figure 1. Examples of precipitation streaks observed by radar.



(a) (b)

Figure 2. Radar observations of horizontal bands.

Drs. Bowen and Wexler are ready to agree that a considerable proportion, but *not all*, of the observed bands may be due to precipitation streaks falling through a region of wind shear as described by Dr. Browne. Certainly, Dr. Marshall has irrefutable evidence in support of the 'streak' theory; his photographs (*J. Met.*, **10**, p. 25, 1953) show clearly the shape of the streaks which appear to originate from generating cells of depth about 4,000 ft and diameter 1 to 2 mi. The origin of these generating elements, which have the appearance of a small Cu on the radar screen, presents a problem; they appear at temperature levels between — 6°C and — 20°C but most frequently between — 13°C and — 15°C. On one occasion, Marshall observed a succession of about twelve such cells with their attendant streaks, spaced at regular intervals of about 20 mi, passing over his radar during a 3-hr period. The computed fall speeds of the particles constituting a streak are rather more than 1 m/sec, which suggests they are snowflakes.

Dr. Bowen says that some of his bands can be interpreted as due to precipitation streaks, but that some cannot. A fair proportion of his upper bands appear to be accurately horizontal, i.e., parallel to the melting band, and do not show the slope which is evident in Marshall's photographs. The absence of slope is, of course, explained if the radar is scanning in a vertical plane at right angles to the wind. Dr. Bowen thinks that his horizontal bands occur rather too frequently for this to be an acceptable explanation.

I think it possible that, under rather special circumstances, the process proposed by Mr. Wexler could take place. But, one consequence of it would be that the intensity of the band should increase rapidly as it falls – this rapid increase is not apparent from Dr. Bowen's film.

Dr. T. W. Wormell : It seems now to be generally agreed that the phenomena being discussed are due, in the great majority of cases, to 'precipitation streaks'; but what is the physical explanation of a precipitation streak ? What is the peculiar property of the small volume, the 'generating element,' which normally travels very nearly with the velocity of the wind at its height, and which continuously produces precipitation so copiously as to cause the familiar streak which may trail twenty or thirty miles upwind ?

Dr. R. S. Scorer : If Dr. Browne makes allowances for the movement through the air of the region generating the precipitation (because it does not consist of the same air all the time) he will have another parameter which he can choose at will to make his observation and theory match with perfection.

Dr. I. C. Browne (*in reply*) : Before replying to the discussion, I should like to raise some matters concerning the theory of upper bands proposed by Dr. Wexler. First, some simple calculations indicate that, if Dr. Wexler is correct, the intensity of an upper band should increase one-hundredfold as the band falls 2,000 ft, whereas observations show that as the band falls this distance, its intensity increases only three-fold or less, a result consistent with the precipitation-streak theory. Second, when one of Dr. Wexler's upper bands reaches the ground, the rainfall rate there should increase for a minute by a factor of about three; in fact, the observed increase in rainfall rate produced by upper bands is an order of magnitude smaller than this. Third, Dr. Wexler's upper bands should be several times deeper than those observed, their depth increasing with time, and they should not show a sharply bounded upper edge. Fourth, if an upper band were the result of a layer of falling graupel, which has a terminal velocity greater than that of snowflakes, the intensity of the upper band should first increase five times on passing through the melting band later returning to double its original value. In fact, as shown in my paper, the observed upper bands, after the five-fold increase in intensity on passing through the melting band, return to their original intensity, a result showing them to be caused by falling ice crystals or snowflakes. Lastly, I should like to question two of Dr. Wexler's assumptions. One of these is that a snow crystal transforms into a pellet of graupel when its rate of growth by accretion equals that by diffusion, the other is that the collection efficiency of an ice crystal has the value 0·8. Surely it would be more reasonable to assume a collection efficiency of the order of 0·1 for flat plates having the size and fall velocity of ice crystals ? Such a value would, of course, be unfavourable to the formation of layers of falling graupel of the kind envisaged by Dr. Wexler.

In reply to Mr. Jones, I am glad to see his excellent examples of upper bands, and to hear that most of them are due to precipitation streaks. I do not deny that horizontal stationary upper bands are observed – I have seen several myself, mostly associated with temperature inversions – but these stationary upper bands are probably quite different in origin from falling or rising upper bands. Mr. Jones's explanation of stationary bands in terms of layers of moist and dry air seems very plausible, but I am puzzled by his remark that Bowen's observation of a stationary band would be explained if the scanning direction were perpendicular to the wind direction. Altering the azimuth of the vertical scanning plane would alter the

observed slope of an upper band, but not the velocity at which it is seen to fall towards the observer.

Mr. Mason has shown some very stimulating photographs taken by Dr. Marshall and his team. He also reports that Dr. Bowen has accurately observed horizontal falling upper bands; the publication of details will be awaited with interest, for certainly none of the upper bands shown in Dr. Bowen's well-known film (shown during the meeting) are horizontal, a fact strongly suggesting that they are all due to precipitation streaks.

Dr. Wormell has put a fundamental question; indeed, Dr. Marshall has suggested that there is no such thing as uniform precipitation above the freezing level in layer cloud, and that all precipitation in such clouds originates in small ' generating elements.' It would be worthwhile to investigate these directly from an aircraft equipped with radar.

I am grateful to Dr. Scorer for the offer of another parameter, though so far it has not been missed. In fact, the horizontal velocity of the cloud relative to the air can be determined very simply from measurements of upper bands and wind speeds. Results have indicated that this relative velocity is zero.

Dr. R. Wexler (*in reply, communicated*) : As Mr. Mason has stated, I agree that a considerable number of the observed bands are due to showers falling through a region of strong wind shear. However, as Dr. Wormell suggests, the bands are more than a problem in geometry, and I believe that rimed crystals and graupel are involved in the formation and dissipation of most of these showers.

In the cloud described by Bowen, bands of heavier precipitation descend in a cloud with fairly uniform light precipitation. The cloud is clearly a stratiform deck penetrated at intervals by cumulus towers. The region of the cloud beneath the cumulus tower is associated with a higher updraught velocity and liquid-water content than in the surrounding stratiform deck. Owing to wind shear the region of high liquid-water content may have a considerable slope. Because of their higher fall velocities, the precipitating particles have a different slope due to wind shear. Some ice particles may spend a major portion of their trajectory within the cumulus tower and layer of high liquid-water content and therefore emerge as graupel. Others experience lighter amounts of riming and maintain their crystalline shape, while within the stratiform deck proper the crystals are unrimed. The descending band of graupel and heavily rimed crystals may diverge, owing to different fall velocities, as they descend and may be further diluted by lightly rimed or unrimed crystals which have had different trajectories. The particles that make up the shower near the 0°C level may therefore consist of graupel, rimed and unrimed crystals. The region of high liquid-water content may be considered as a transitory phenomenon, occasionally produced by a surge of updraught and depleted, after a period of 10 to 20 min, by riming, Hence the principle of the formation and disappearance of these bands may be described in my paper, although their descent may be as described by Dr. Browne.

Dr. Browne misquotes one of my assumptions. I assumed that ice crystals transform into graupel *soon after* the rate of growth by accretion exceeds that by diffusion (not when they are equal). It is easy to show that the rate of growth by accretion increases much more rapidly than that by diffusion. Anyone who has made comparative computations will realize the validity of the assumption.

Dr. Browne also questions a collection efficiency of 0·8 for ice crystals. Referring to Langmuir and Blodgett (' A mathematical investigation of water-droplet trajectories,' *A.A.F. Tech. Rep.*, No. 5418, 1946), a value of $K = 2{\cdot}3$ corresponds to a crystal of radius 0·2 mm falling at about 35 cm/sec through a cloud of 10 μ-radius drops; from their Figs. 1, 10, and 13, we find the following collection efficiencies for potential flow :

cylinders, 0·59; ribbons, 0·83; spheres, 0·65.

Since a ribbon has a greater collection efficiency than a cylinder for the same K value, it is not unreasonable to expect that a disc, to which a snow crystal may be approximated, has a greater collection efficiency than a sphere. Hence $E = 0{\cdot}8$ is a valid approximation. Is there any physical basis for Dr. Browne's feeling that $E = 0{\cdot}1$ is more valid ? It is true that larger dendritic crystals have lower collection efficiencies but my article was perfectly clear on that point.

Report of the Council

for the Year 1952

(Submitted to the Annual General Meeting, 15 April 1953)

Royal Patronage

After the death in February of our Patron, His late Majesty King George VI, petition was made to Her Majesty Queen Elizabeth II for the grant of her patronage. It is with pleasure that Council record that Her Majesty graciously consented to become the Society's Patron.

General

The Council are glad to be able to report that there has been further steady progress during the year 1952, and that for the second year in succession there has been an increase over the previous year in the number of Fellows and Foreign Members of the Society; the total number of Members was the highest in our records.

A further generous grant was received from the Munitalp Foundation for the purpose of promoting co-operation and goodwill between the Society and the American Meteorological Society.

The third vacation course, held at Malham Tarn Field Centre in conjunction with the Council for the Promotion of Field Studies, was very successful and 12 of the students were subsequently elected to membership of the Society.

Council

From 1 January to 30 September the constitution of the Council was as recorded in the 1951 Annual Report, and from 1 October 1952 the following Council took office :

President

Sir Charles Normand, C.I.E., M.A., D.Sc.

Vice-Presidents

Professor Emeritus Sir David Brunt, Sec.R.S.
Wg.-Cdr. Robert Martin Poulter, O.B.E.
Sir Robert Watson-Watt, C.B., F.R.S.
William Moody Witchell, B.Sc., F.R.A.S.

Vice-President for Canada

Andrew Thomson, O.B.E., M.A.

Vice-President for Scotland

Edmund Gilbert Dymond, M.A., F.R.S.E.

Treasurer

James Strachan Farquharson, M.A., D.Sc.

Secretaries

Professor Percival Albert Sheppard, B.Sc., F.Inst.P.
Richard George Veryard, B.Sc.

Editor

Professor Percival Albert Sheppard, B.Sc., F.Inst.P.

Councillors

Donovan Chilton, B.Sc.
David Duncan Clark, M.A.
Percy Robert Crowe, Ph.D.
Mrs. Ann Douglas
Arthur George Forsdyke, Ph.D., D.I.C., A.R.C.S.
Richard Mead Goody, M.A., Ph.D.
Lt. Cdr. George Lawrence Hogben, R.N.Z.N., D.S.C., M.A., D.I.C., Ph.D.
Ralph Edwin Lacy, B.Sc.
Frank Henry Ludlam, D.I.C.
Cameron Darrell Ovey, B.Sc., F.G.S.
George David Robinson, Ph.D., F.Inst.P.
Henry Lionel Wright, M.A.

The following changes in Council occurred after 1 October 1952 : Dr. A. E. M. Geddes was elected Chairman of the Scottish Centre and Vice-President for Scotland, on the death of Mr. E. G. Dymond; Mr. H. L. Wright resigned from Council in October on being posted abroad, and Mr. Henry Charnock was elected by Council in his place. Both elections took place under By-law 52.

Meetings

The Annual General Meeting was held at the Society's rooms on 23 April. The Report of the Council and the Annual Accounts for 1951 were adopted. The Presidential Address was also given on 23 April by Sir Charles Normand, C.I.E., M.A., D.Sc., his subject being 'Atmospheric ozone and the upper-air conditions.'

The Symons Memorial Lecture was delivered on 19 March by Dr. M. V. Wilkes who spoke on 'World-wide oscillations in the earth's atmosphere.' Six Ordinary Meetings were held during the year at the Society's rooms, and the Summer Meeting took place on 16 July at London Airport.

The Society's Second Annual Dinner was held on 27 November. The theme of the after-dinner speeches was Meteorology and Agriculture and the Guest of Honour was Professor Sir James Scott Watson, C.B.E., LL.D., M.C., Chief Scientific and Agricultural Adviser of the Ministry of Agriculture and Fisheries, and Director General of the National Agricultural Advisory Service. Prominent personalities in the agricultural world were present and the principal agricultural and horticultural organizations were represented.

Additional events of interest were : Discussion meeting on 'Meteorology and the operation of jet aircraft' on 20 February; annual popular lecture on 'The upper atmosphere – from 10 to 100 miles up' by Dr. G. M. B. Dobson, C.B.E., F.R.S., on 11 December.

Reports of the proceedings were published in the *Quarterly Journal* and *Weather*.

Publications

The inflow of scientific communications for the *Quarterly Journal* and *Weather* was well maintained. 74 papers were submitted for publication in the *Quarterly Journal* during 1952, as against 61 in 1951. 16 of the papers were either withdrawn by the authors or rejected. The average time between receipt of approved copy and publication in the *Journal* was about 4 months.

The four numbers of the 1952 *Journal* together contained 666 pages, and the series 'Reviews of modern meteorology' were continued in each number.

Council are greatly indebted to the Editor, Professor P. A. Sheppard, the Editing Committee and the proof readers for their services in the production of the *Journal*.

Owing to the continuing rise in production costs it was found necessary in July to increase for the first time since the commencement of publication in 1946 the price of the Society's monthly magazine *Weather*. Grateful thanks are due to the Editors of *Weather* for their sustained efforts in the production of this very popular magazine.

Membership

Table A

	Fellows and Foreign Members			Associates	Students	Honorary Members	TOTAL
	Annual	Life	Total				
31 Dec. 1951	1,259	186	1,445	301	84	16	1,846
Since elected	130	1	131	18	24	—	173
Readmitted	3	—	3	—	—	—	3
Transferred	+ 10	− 1	+ 9	+ 1	− 12	+ 2	—
Deceased	− 17	− 3	− 20	− 1	—	—	− 21
Resigned, etc.	− 89	—	− 89	− 12	− 6	—	− 107
31 Dec. 1952	1,296	183	1,479	307	90	18	1,894

Deaths

Council record with sorrow the death of the Senior Fellow of the Society, Dr. A. C. Crichton-Mitchell, who was elected to membership of the Scottish Meteorological Society in 1891. He served as a Secretary and Vice-President of the Society. The deaths of Mr. E. G. Dymond (elected in 1946), while Vice-President for Scotland, and of Sir Richard Gregory, who was elected in 1926 and who served as President from 1928-30, are also recorded with deep regret.

The following deaths are also regretfully recorded, the dates of election being appended to the names : G. A. B. Aubrey (1951), Flt. Lt. A. W. Batchelor (1950), Capt. T. R. Dow (1930), J. S. Ellis (1918), Rev. H. W. C. Geldart (1930), H. W. Halbert (1947), Dr. E. H. Howell (1946), Capt. J. L. Keen (1949), Flg. Off. C. G. Keyte (1949), Col. H. W. Mirehouse (1949), Sir Michael Nairn (Life Fellow, 1919), A. M. Peters (Life Fellow, 1925), J. H. Reynolds (1933), Rev. Canon E. F. Robson (1924), R. G. C. C. Sandeman (1933), H. G. Walker (1912), Dr. R. E. Watson (1921), Sir John Wood (1930).

Library

The work of reorganization has steadily progressed during the year. The reading room on the first floor has been fitted out as a Reference and Research Room. Mr. R. S. Read, Assistant Librarian, who had hitherto worked on a part-time basis, was appointed full-time Assistant Librarian on 1 September.

The thanks of the Council are due to the Librarian, Dr. G. D. Robinson, for the considerable work he has done in the reorganization.

Staff

At the close of the year the staff consisted of Mr. M. E. Shoenberg, assisted by : Miss V. C. Jones, *Weather* Clerk; Mr. R. S. Read, Assistant Librarian; Mrs. A. Wallis, Accounting Assistant; Miss A. Dent, Junior Clerk; together with the part-time services of Miss J. Sampson, Secretarial Assistant; and Mrs. W. Steward, Publications Clerk. Their services to the Society have been very valuable.

Branch and Centres

Canadian Branch

Nine Meetings were held during the year at Toronto, one being the Annual Business Meeting and the others for lectures and presentation of papers. Seven lectures or papers were reproduced as Canadian Branch publications. The Branch continued to prosper and finished the year with a record membership of 350. Preparation for the Joint American Meteorological Society-Royal Meteorological Society Scientific Meeting in 1953 formed one of the principal activities of the Executive. The Council wish to record their very great appreciation of the work of the Canadian Executive Committee who so readily gave their services. The members of the committee were : President, F. W. Benum; Vice-President, D. C. Archibald; Secretary, R. C. Graham; Treasurer, A. H. Mason; Councillors at Large, D. N. McMullen, W. R. Fryers, D. P. McIntyre, J. M. Leaver, P. Johns; Vice-President for Canada, A. Thomson.

Scottish Centre

Five Meetings were held during the year, one of which was addressed by the President; four were at Edinburgh University and one an excursion to the Ocean Weather Ship Base at Greenock. The Officers were : Chairman and Vice-President for Scotland, Sir Ernest Wedderburn (1 January to 30 September), followed by Mr. E. G. Dymond; Secretary and Treasurer, Mr. J. Paton.

After Mr. Dymond's death on 26 October, Dr. A. E. M. Geddes was elected Chairman and Vice-President for Scotland.

Midland Centre

Five Meetings were held during the year, one of which was addressed by the President and another consisting of a visit to the Elan Valley Waterworks. The Officers during 1952 were : Chairman, A. L. Kelley; Secretary, P. B. Coley; Treasurer, H. E. Howle.

Representatives

The following is a list of the Society's representatives on other scientific bodies:

Royal Society Committees

Gassiot Committee, President; *Scientific Centre Accommodation Committee*, Sir Robert Watson-Watt; *British National Committee for Geodesy & Geophysics*, E. Gold (1949-54), O. G. Sutton (1952-57); *British National Committee for Physics*, F. J. Scrase (1949-54).

Institute of Physics Committees

Board, G. D. Robinson (1949-53); *Journal of Scientific Instruments Advisory Committee*, F. Pasquill; *British Journal of Applied Physics Advisory Committee*, J. S. Forrest.

Cambridge University Meteorological Committee, P. A. Sheppard, January-September; President, October-December.

Royal Astronomical Society

Geophysical Committee, R. C. Sutcliffe.

Scientific Film Association, P. J. Meade.

Advisory Committee for the Meteorological Office, Edinburgh, J. Paton.

Awards

Buchan Prize

To Dr. G. D. Robinson for his outstanding contributions to the study of atmospheric radiation.

Hugh Robert Mill Medal and Prize

To Dr. J. Glasspoole for his outstanding contributions to the science of meteorology with particular reference to rainfall.

Darton Prizes (in respect of 1951)

1st Prize : Mr. J. M. Craddock, for his paper ' An apparatus for measuring dewfall ' (*Weather*, p. 300, 1951).

2nd Prize : Jointly to Dr. E. G. Bowen and Mr. K. A. Davidson, for their paper ' A raindrop spectrograph ' (*Q.J.*, **77**, p. 445, 1951).

Canadian prizes were also instituted during the year and were awarded as follows on the recommendation of the Canadian Branch :

1st Prize : Mr. F. E. Burbidge, for his paper ' The modification of continental polar air over Hudson Bay ' (*Q.J.*, **77**, p. 365, 1951).

2nd Prize : Dr. W. L. Godson, for his paper ' Synoptic properties of frontal surfaces ' (*Q.J.*, **77**, p. 633, 1951).

The awards were presented at the Annual General Meeting on 23 April.

During the year grants were made out of the Scientific Activities Fund to :

Mr. W. E. Richardson, in respect of instruments required for climatological observations on Park Fell, near Alston, Cumberland.

Mr. J. A. Taylor, in respect of instruments required for an investigation of soil and air temperatures on different soil types in SW. Lancashire.

Mr. F. H. Ludlam, in respect of a visit during September 1952 to Prof. Tor Bergeron, Department of Synoptic Meteorology, University of Uppsala.

Dr. R. S. Scorer, in respect of attending the biennial meeting of Organisation Scientifique et Technique Internationale du Vol à Voile, Madrid, where he read papers and also acted as meteorological adviser to the British gliding team in the International Gliding Contests held at the same time.

Donation

The Society has received a further grant of $3,750 from the Munitalp Foundation and the Council wish to record once more their gratitude for this generosity. The gift will be used for furthering the Society's activities.

Finance

Attention was drawn in the Report for 1951 to the modified form of presentation of the accounts. The simplification has been carried further this year by giving all figures to the nearest £; also for the first time comparative figures are given in respect of the investments of the Society.

The continued growth in membership of the Society and the popularity of its publications as indicated elsewhere in the Report are reflected in the satisfactory financial position.

During the year approximately £200 was spent in modernizing and equipping the library.

£536 (as compared with £403 in 1951) has been received as a refund from the Inland Revenue in respect of seven-year deeds of covenant signed by Fellows for payment of their subscriptions. The attention of Fellows is drawn to the substantial benefits accruing to the Society under this scheme. It is hoped that more Fellows will be encouraged to participate.

Council record with gratitude an allocation to the Society by the Royal Society Committee of £1,300 from the Government grant-in-aid of publications fund, with an addition of £200 for the library.

STATEMENT OF RECEIPTS AND PAYMENTS

RECEIPTS

1951 £	£			£	£
		Balance from 1951—			
		Cash on Deposit at Westminster Bank		750	
		Cash on Current Account		259	
		Cash on Deposit at Post Office Savings Bank		544	
107					1553
	3820	Subscriptions 1952		3617	
	174	,, former years		345	
	222	,, 1953 and 1954		201	
4216					4163
46		Donations from Life Members, etc.			37
128		Entrance fees			132
98		Interest on investments			147
		Library—			
	150	Grant from Royal Society (including £50 voted in 1951)		250	
	1750	Sale of old Books		235	
	1900			485	
		Less expenditure on—			
	21	Equipment and shelving	188		
	83	Books and Binding, etc.	82		
	104 —		—	270	
1796					215
	3011	Sale of Publications		2752	
		Advertisements		137	
	571	*Centenary Proceedings*		163	
3582					3052
1050		Grants made by the Royal Society (excluding Library)			1300
403		Income Tax refunds from covenanted subscriptions			536
1783		Donation from Munitalp Foundation			1343
	319	Rent of Maisonette		303	
	121	*Less*—Rates	122		
	—	—Repairs	6		
	121 —		—	128	
198					175
		Sale of cloud postcards		31	
	153	Sale of Clarke and Cave reproductions	53		
	10	*Less* expenses	4		
143			—	49	
					80
6		*Centenary Special Appeal*		—	
		Transferred from other Funds—			
	175	Life Composition Fund		200	
	—	British Rainfall Organization Fund		100	
	1	Repairs and Maintenance Fund		—	
176					300
80		Amounts due to other Funds			2
13812					£13035

12 March 1953

FOR THE YEAR ENDING 31 DECEMBER 1952

PAYMENTS

1951 £	£		£	£
		Publications, etc.—		
	2473 (4 numbers)	*Quarterly Journal*, 5 numbers Oct. 1951 to Oct. 1952	3878	
	1060	*Centenary Proceedings*	—	
	182	Illustrations	227	
	331	Reprints of Papers	348	
	15	Reviews	23	
		Payments to *Weather* for Members' copies (including £500 for 1951)	1030	
	78	Second-hand Journals	289	
4139				5795
		Printing—		
	100	General printing	119	
	406	Stationery and Bookbinding	243	
	58	*Monthly Weather Report*	61	
564				423
		Office Expenses—		
	1774	Salaries and Insurance (excluding Library, including *Weather* 1951 refund of £347)	1438	
	397	Salaries and Insurance, Library Staff	459	
	242	Cleaners and Insurance	277	
	336	Furniture	92	
	26	Auditing Accounts for 1951	26	
	62	Telephone charges	56	
	83	Petty expenses	57	
	696	Packing, Postage and Carriage	768	
	77	*Solicitors' fees*	—	
	900	Modernizing Society's Offices	87	
4593				3260
		Heating, Lighting, Insurance, etc.—		
	53	Coal and coke	42	
	71	Light and gas	102	
	12	Water rate	12	
	20	Insurance	18	
156				174
		Meetings—		
	133	Expenses and refreshments at Meetings	120	
	35	Scottish Centre, Meetings expenses	40	
	—	Midland Centre, Meetings expenses	15	
168				175
342		Payments on behalf of *Weather*		4
		Purchase of Investments—		
687		£1,000 2½% Consolidated Stock		—
		Transferred to other Funds—		
	100	British Rainfall Organization Fund	2	
	1016	Scientific Activities Fund (from Munitalp Foundation £269)	299	
	—	Life Composition Fund	43	
	191	Staff Pension Fund	—	
	—	Napier Shaw Fund	5	
	303	Repairs and Maintenance Fund	57	
1610				406
		Cash in Hand—		
	750	Cash on deposit, Westminster Bank Ltd. (balance of Munitalp Donation, 1952)	1074	
	259	Cash in current account	657	
	544	Cash on deposit at Post Office Savings Bank	1067	
1553				2798
13812				£13035

Examined, compared with vouchers, and found correct,

Spencer House, South Place, London, E.C.2. BALL, BAKER & Co.,

Chartered Accountants.

ASSETS AND LIABILITIES AT 31 DECEMBER 1952

1951 £	1951 £	LIABILITIES	£	£
	222	Subscriptions paid in advance	201	
	187	Subscriptions to publications paid in advance	498	
409				699
	672	Accounts outstanding – *Quarterly Journal* and Reprints	—	
	—	Rebate payable on Canadian subscriptions	106	
	878	Sundry Expenses	50	
		Due to Staff Pension Fund	450	
		Due to Life Composition Fund ..	100	
		Provision for special publications ..	200	
1550				906
9840		Excess of Assets over Liabilities (the excess is exclusive of the value of the Library and stock of publications) ..		11297
11799				£12902

1951 £	1951 £	ASSETS	£	£
	633	Government of Australia 4% Registered Stock 1955-70 £649.1.7. @ 96½ xd ..	626	
	197	4% Funding Stock 1960-90 £200 @ 98½	197	
	738	3½% Conversion Loan £900 @ 79 ..	711	
	861	2½% Consolidated Stock £1400 @ 58½ xd	819	
2429				2353
		The above stocks are valued at market prices on 31.12.52 by the Society's brokers.		
	343	Due from *Weather*	—	
	450	Subscriptions unpaid (including £707 Canadian subscriptions, since received)	850	
	—	Accounts due for Clarke Collection ..	15	
	890	Accounts due for Publications	404	
	—	Payment in advance for Jan. 1953 *Journal*	47	
1683				1316
	738	Furniture and Fittings—General ..	830	
	—	—Library ..	209	
	738		1039	
	16	Instruments	16	
	5375	Estimated value (in 1925) of the Society's freehold house, 49 Cromwell Road ..	5375	
6129				6430
	5	Deposit with London Electricity Board..	5	
	544	Deposit with Post Office Savings Bank ..	1067	
	1009	Balance, cash at Westminster Bank Ltd. and in hand	1731	
1558				2803
11799				£12902

12 March 1953

Examined and securities seen,
BALL, BAKER & Co.,
Chartered Accountants.

RECEIPTS AND EXPENDITURE OF SPECIAL FUNDS FOR 1952

	Balance at 31.12.51	Income for 1952	Expenditure for 1952	Balance at 31.12.52
	£	£	£	£
Awards Fund	17	—	—	17
British Rainfall Organization Fund	96	180	(c) 190	86
Life Composition Fund	185	(a) 280	(d) 200	265
Napier Shaw Memorial Fund	70	22	—	92
Repairs and Maintenance Fund	—	57	57	—
Research and Special Purposes Fund	162	—	(e) 162	—
Royal Society Reprints Scheme	3	35	29	9
Scientific Activities Fund	20	(b) 506	208	318
Staff Pension Fund	58	516	(f) 60	(g) 514
Symons Memorial Fund	40	31	16	55
	£651	£1627	£922	£1356

(a) Includes £43, transferred from General Account and £100 due from General Account; (b) Includes £162 transferred from Research and Special Purposes Fund and £269 from the Munitalp Foundation Donation; (c) Includes £100 transferred to General Account in repayment of loan; (d) £200 transferred to General Account ; (e) Fund amalgamated with Scientific Activities Fund; (f) Purchase of £100 2½% Consolidated Stock; (g) Includes £450 due from General Account.

12 March 1953

Examined and found correct,
BALL, BAKER & Co.,
Chartered Accountants

VALUATION OF INVESTMENTS AT 31 DECEMBER 1952

Market Value 31.12.51			*Market Value 31.12.52*	
£	£		£	£
		*General Fund**		
633		£649.1.7 Government of Commonwealth of Australia 4% Registered Stock 1955-70 ..	626	
197		£200 4% Funding Stock 1960-90	197	
738		£900 3½% Conversion Loan 1961 or after	711	
861		£1,400 2½% Consolidated Stock	819	
2429				2353
		Scientific Activities Fund		
203		(*a*) £250 3½% War Stock 1952 or after	195	
82		(*a*) £100 3½% Conversion Loan 1961 or after	79	
900		£1,000 4% Consolidated Stock 1957 or after ..	870	
1185				1144
		Staff Pension Fund		
648		£800 3½% War Stock 1952 or after	624	
246		£300 3½% Conversion Loan 1961 or after	237	
646		(*b*) £1150 2½% Consolidated Stock	673	
1540				1534
		Life Composition Fund		
492		£800 2½% Consolidated Stock	468	
656		£800 3½% Conversion Loan 1961 or after	632	
1200		£1,200 3% Defence Bonds	1200	
2348				2300
		*British Rainfall Organization Fund***		
607		£750 3½% War Stock 1952 of after	585	
100		£100 3% Defence Bonds	100	
2401		£3,097.10.0 3% British Transport Guar. Stock 1978-88	2385	
3108				3070
		Napier Shaw Memorial Fund		
287		£350 3½% Conversion Loan 1961 or after		276
		Symons Memorial Fund		
533		£650 3½% Conversion Loan 1961 or after ..	514	
180		£200 4% Consolidated Stock 1957 or after	174	
713				688
11610				£11365

(*a*) Formerly Research and Special Purposes Fund. (*b*) £100 2½% Consolidated Stock purchased in 1952.

* *General Fund* also holds the freehold of 49 Cromwell Road, S.W.7.

** *British Rainfall Organization Fund* also holds the freehold of 62 Camden Square, N.W.1.

12 March 1953

Examined and found correct,
BALL, BAKER & Co.,
Chartered Accountants.

Proceedings at meetings of the Society

Ordinary Meeting, 18 *March* 1953

The following candidates were declared to have been elected that day by the Council :

Fellows

Akingbehin, Fg. Off. Nathaniel Akindele, Rose Cottage, 84 Strand-on-the-Green, Chiswick, London, W.4. (permanent address : 21 Oshodi Court, Oshodi Street, Lagos, Nigeria).
Ambrose, Capt. Lewis Robert, Flat 1, 40 Hyde Park Gate, London, S.W.7.
Chandler, Gordon Mountford, Gorse Cottage, Hartfield, Sussex.
Chown, John Bertram Norman, 17 Claremont, Alloa, Clackmannanshire.
Mundell, George Turner, Dusseldorf-Lohausen, Flughafen, Dusseldorf, Germany.
Olafson, Raymond Albert, Aviation Forecast Office, R.C.A.F. Station, Saskatoon, Saskatchewan, Canada.
Parker, Graham Howard, 61 All Souls Avenue, Willesden, London, N.W.10.
Pinchin, Ralph Donald, B.Sc., D.I.C., Alice Holt Lodge, Farnham, Surrey.

Foreign Members

Kussman, Abraham S., 70 Neptune Avenue, New Rochelle, N.Y., U.S.A.
Miller, Prof. James E., New York University, New York 53, N.Y., U.S.A.
Mispelblom Beyer, Hendrikus Albertus Diederikus, 17 Dorpsstraat, Warmond, Netherlands.
Purdy, Donald R., Weather Bureau ACN Project, Naval Air Station, Seattle 5, Wash., U.S.A.
Sanders, Frederick, 92 Front Street, Marblehead, Mass., U.S.A.
Thompson, Asst. Prof. Aylmer H., M.A., Department of Meteorology, 312 Mines Building, University of Utah, Salt Lake City 1, Utah, U.S.A.

Associates

Ayers, Fg. Off. John M., 81 Riverdale, Ottawa, Ontario, Canada.
Bishop, Ronald Horace, Aviation Forecast Office, R.C.A.F. Station, Trenton, Ontario, Canada.
Bitcon, Robert, Officers' Mess, R.C.A.F. Station, Claresholm, Alberta, Canada.
Clark, Kenneth Hector, Aviation Forecast Office, R.C.A.F. Station, Chatham, New Brunswick, Canada.
Cooper, Arthur Burton, Meteorological Office, Maritime Group Headquarters, R.C.A.F., 17 South Street, Halifax, Nova Scotia, Canada.
Dow, Alvin Donald, Aviation Forecast Office, R.C.A.F. Station, Winnipeg, Manitoba, Canada.
Ferguson, Howard Leslie Barbour, Aviation Forecast Office, R.C.A.F. Station, Trenton, Ontario, Canada.
Gilmour, William David King, c/o Aviation Weather Office, R.C.A.F. Station, Trenton, Ontario, Canada.
Gladstone, Alexander, 4105 De Bullion Street, Montreal, P.Q., Canada.
Goritz, John, Meteorological Office, P.R.T.S., R.C.A.F., c/o 25th A.M.B., Currie Field, Calgary, Alberta, Canada.
Grundy, Norman Alan, District Aviation Forecast Office, Malton Airport, Malton, Ontario, Canada.
Hill, Richard Harry Wooding, Aviation Forecast Office, R.C.A.F. Station, Portage la Prairie, Manitoba, Canada.
Howse, Gordon Ernest, Aviation Forecast Office, R.C.A.F. Station, Centralia, Ontario, Canada.
Jarvis, Edgar Clive, Aviation Forecast Office, R.C.A.F. Station, Winnipeg, Manitoba, Canada.
Juby, Maxwell Lloyd, Officer-in-Charge, Dependent Forecast Office, Seven Islands, Quebec, Canada.
Karpman, Jack, Officers' Mess, R.C.A.F. Station, Trenton, Ontario, Canada.
Kruger, Herbert Bertholt, Aviation Forecast Office, R.C.A.F. Station, Bagotville, Quebec, Canada.
Lyngseth, Dalmar Milton, Aviation Forecast Office, Fort Churchill, M.P.O. 1015, via Winnipeg, Manitoba, Canada.
MacMillan, Lionel Walter, R.C.A.F. Station, Officers' Mess, North Bay, Ontario, Canada.
McDonald, Robert Barrie, Aviation Forecast Office, R.C.A.F. Station, Fort Nelson, British Columbia, Canada.

McCulloch, James A. H., 420 Brunswick Avenue, Toronto, Ontario, Canada.
Muttitt, Gordon Harold, Department of Transport, Goose Airport, Labrador, via Montreal, Quebec, Canada.
Neuwelt, Michael Anthony, Aviation Forecast Office, R.C.N. Air Station, Dartmouth, Nova Scotia, Canada.
Nordstrand, Norman Peter, L.A.C., M.P.O. 1015, R.C.A.F., Fort Churchill, Manitoba, Canada.
Pyra, Stanley Joseph, Meteorological Office, P.R.T.S., R.C.A.F., c/o 25th A.M.B., Currie Field, Calgary, Alberta, Canada.
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Reimer, Abram, Aviation Forecast Office, R.C.A.F. Station, Winnipeg, Manitoba, Canada.
Ring, Jacob William, Main Meteorological Office, Trans-Atlantic Administrative Building, Montreal Airport, Dorval, Quebec, Canada.
Scott, Donald McGeachy, Aviation Forecast Office, R.C.N. Station, Dartmouth, Nova Scotia, Canada.
Slater, Donald F. A., 41 George V Avenue, Dorval, P.Q., Canada.
Snow, Donald G., 9 Murray Place, Halifax, Nova Scotia, Canada.
Thomson, J. A. G., 113 St. George Street, Toronto, Ontario, Canada.
Watkin, Ronald William, c/o Aviation Forecast Office, R.C.A.F. Station, Centralia, Ontario, Canada.
Watson, Warren Blake, Aviation Forecast Office, R.C.A.F. Station, Portage la Prairie, Manitoba, Canada.
Wingfield, John M., Meteorological Office, 315 Bloor Street West, Toronto 5, Ontario, Canada.
Yacowar, Nathan, 4594 Decarie Boulevard, Apartment 17, Montreal, P.Q., Canada.

Student Members

Carlisle, Robert W., 165 Deerpark Road, Belfast.
Jackson, Michael, 15 Porlock Road, Flixton, Near Manchester.
Noble, John Wilkinson, c/o Stuart, 1 South Oxford Street, Edinburgh, 8.
Oliver, Anne C., 87 Grange Loan, Edinburgh 9.
Stanley-Clarke, Bryan Graeme, Rossearn, Comrie, Perthshire.

The following papers were read and discussed :

(1) ' The growth of ice crystals in a supercooled water cloud ' by B. J. Mason (*Q.J.*, **79**, p. 104).

(2) (a) ' Theory of the radar upper band ' by R. Wexler (*Q.J.*, **78**, p. 372). The paper was presented by Mr. E. K. Bigg.

(b) ' Precipitation streaks as a cause of radar upper bands ' by I. C. Browne (*Q.J.*, **78**, p. 590).

Annual General Meeting, 15 *April* 1953

Wg. Cdr. R. M. Poulter, O.B.E., Vice-President, in the chair.

The chairman announced with regret the absence of the President on account of the sudden death of Lady Normand. The meeting recorded its deep sympathy with Sir Charles Normand in his great loss.

The chairman explained that as the Presidential Address had had to be postponed, Council had accepted Professor Gordon Manley's kind offer to speak on ' Field evidence of climatic change.'

The minutes of the Annual General Meeting on 23 April 1952 were confirmed and signed.

In presenting the Report of the Council for the year 1952, the chairman referred to the main items of interest and drew attention to the steady increase in membership which he felt was largely due to the high standard of the *Quarterly Journal*, and the popularity of the monthly magazine *Weather*. The total membership of 1,894 was the highest on record and showed an increase of 1,000 since 1942.

He also referred to the second gift from the Munitalp Foundation which would be used for the expenses of the Society's delegates to a joint meeting with the American Meteorological Society in Toronto in September 1953.

The Hon. Treasurer presented the statement of accounts for the year and drew attention to the form of presentation which had been further simplified. He referred to the increased printing costs of the *Quarterly Journal*, but pointed out that the finances of the Society were in a strong position.

The chairman moved that the Report be received and adopted and be printed in the *Quarterly Journal*. The motion was carried unanimously.

Mr. E. Gold paid a warm tribute to the late Lady Normand, and then proposed the vote of thanks to the President and Council for the year 1952. This was seconded by Mr. H. W. L. Absalom.

The chairman then announced the following awards :

(a) *Symons Memorial Gold Medal* for 1953 :

To Professor Carl-Gustav Rossby in recognition of his outstanding contributions to meteorology.

He has placed his permanent impress on the fundamental branches of the science from small-scale turbulence of the lower atmosphere to the dynamics of the large-scale currents and their relation to the general circulation of the atmosphere.

We esteem Professor Rossby most of all perhaps for the vigorous lead he has given to scientific meteorological research and for his inspiration and encouragement of meteorologists in all parts of the world.

The medal was then presented to His Excellency the Swedish Ambassador, who received it on behalf of Prof. C.-G. Rossby, and read the following message of thanks from Prof. Rossby :

Please convey to the Council of the Royal Meteorological Society my deep gratitude for the great honour conferred upon me through the award of the Symons Memorial Gold Medal for 1953.

My collaborators and I would also like to thank you for the strong support our research programme in Stockholm has received from British meteorologists. Our efforts to develop techniques for the use of electronic computors in meteorological calculations, and particularly our first steps to develop a rational research programme in cloud physics, are enormously strengthened by the outstanding contributions made by British scientists who from time to time have worked with our group. The facilities and meetings ground for international contact provided by our Institute are always open to our British colleagues. In the near future we hope to be able to send several of our young people to work in the stimulating environment provided by British meteorological institutions.

(b) *Darton Prizes* for Canadian Members 1952 :

(1) Dr. D. P. McIntyre, for his paper 'A technique for wind analysis and forecasting as applied to Victoria, B.C.' (*Quarterly Journal*, **78**, p. 247, 1952);

(2) Mr. F. W. Benum, for his paper ' Meteorology and aviation ' (Canadian Branch publications, **3**, No. 1).

Prof. G. Manley then gave his talk on ' Field evidence of climatic change ' which he illustrated with lantern slides.

The meeting cordially thanked Prof. Manley for giving such an interesting and stimulating talk at very short notice.

The meeting was declared closed at 6.50 p.m.

Symons Memorial Lecture, 29 April 1953

Dr. T. W. Wormell delivered the Symons Memorial Lecture, his subject being ' Lightning.' This was followed by a discussion. The Lecture and discussion will be published in a subsequent issue of the *Quarterly Journal*.

Ordinary Meeting, 20 May 1953

The following candidates were declared to have been elected by the Council on 15 April :

FELLOWS

BAMBER, Capt. Vivian Gordon, The Cottage, 13 Barton Road, Bournemouth, Hants.
MCGAIN, Robert Edward Stanley, ' St. Martin's,' 28 Church Road, Alphington, Exeter, Devon.
MATHUR, Lakshmi Sahay, M.Sc., D.Phil., c/o The British Council, 65 Davies Street, London, W.1. (permanent address : The Observatory, New Delhi, India).
SNELL, Instr. Lt. Raymond William, R.N., B.Sc., Grad.Inst.P., H.M.S. Glory, c/o G.P.O., London.
VINE, Instr. Lt. Leslie Lavey, R.N., B.Sc., Lydford, 40 Glenleigh Avenue, Bexhill-on-Sea, Sussex.

FOREIGN MEMBERS

BYERS, Prof. Horace R., Sc.D., Department of Meteorology, University of Chicago, Chicago 37, Ill., U.S.A.

STUDENT MEMBERS

NEWMARK, David Michael, ' Sefton,' Dartry Road, Rathmines, Dublin.
SMITH, Stanley Graham, Meteorological Bureau, R.A.A.F. Station, Darwin, N.T., Australia.

The following candidates were then declared to have been elected that day by the Council :

FELLOWS

AGIUS, Anthony, 150 St. Joseph High Road, Hamrun, Malta.
BAIRD, David Carr, Royal Military College of Canada, Kingston, Ontario, Canada (*transfer from Student Membership*).
COLES, Victor Roy, M.Sc., Grangewood, 9 Stanhope Close, Wendover, Bucks.
FELL, William Henry, Chief Navigator, Vickers-Armstrongs Ltd., Wisley, Ripley, Surrey.
HIDAYETULLA, Sqn. Ldr. M. S., Chief Meteorological Officer, Air Headquarters, Karachi, Pakistan.
LIBGOTT, Capt. Wilf, Berkenrodelaan 3, Amsterdam Z (II), Holland.
MOLLOY, Vernon, Officers' Mess, R.A.F. Bückeburg Airfield, 2nd T.A.F., B.A.O.R. 29.
PETERSON, Peter, M.Sc., D.I.C., Meteorological Office, 6th Floor, Fullerton Building, P.O. Box 715, Singapore 1 (*transfer from Student Membership*).
REYNOLDS, Geoffrey, ' Bradda,' Raeburn Avenue, West Kirby, Cheshire.
SHEPHARD, Christopher Leonard, 29 Idmiston Road, London, S.E.27.
VERRAN, Flt. Lt. Gordon Leslie, University of London Air Squadron, R.A.F. Booker, Marlow, Bucks.
VICKERS, Michael Kenneth, M.A., The Grange, Worton, Devizes, Wilts.
ZAKAULLAH, Capt. Mohammed, 35 Blagdon Avenue, South Shields, Co. Durham (permanent address : Mercantile Marine Dept. (Govt. of Pakistan), Custom House Building, Karachi, Pakistan).

FOREIGN MEMBER

COLÓN, José A., B.S., M.S., Department of Meteorology, The University of Chicago, Chicago 37, Ill., U.S.A.

ASSOCIATE

ROAKE, Harold Stuart, P.O. Box 252, Asmara, Eritrea.

STUDENT MEMBER

WELBURN, Miss Dorothy Margaret, Lindsell Hall, 35 Adamson Road, London, N.W.3.

The following papers were read and discussed :

(a) ' Annual recurrences in Edinburgh temperature ' by D. H. McIntosh (*Q.J.*, **79**, p. 262).

(b) ' Climatic fluctuation in the Greenland and Norwegian seas ' by P. R. Brown (*Q.J.*, **79**, p. 272).

(c) ' The mean temperature of central England 1698-1952 ' by G. Manley (*Q.J.*, **79**, p. 242).

Reviews

Micrometeorology. By O. G. Sutton. New York (McGraw-Hill), 1953. Pp. xii, 333; 35 Figs., 30 tables. 61s.

For many years the only substantial text-book of micrometeorology has been Geiger's famous *Das Klima der bodennahen Luftschicht* and, as the title implies, the strength of that work lies in description rather than explanation. Professor Sutton's book is subtitled ' A study of physical processes in the lowest layers of the earth's atmosphere ' ; it concentrates on those aspects on which ' Geiger ' is weakest and does not attempt to compete with it in descriptive detail. The preface says ' the subject matter and treatment are mainly of postgraduate level ' but it is not likely that a second- or third-year student of physics or mathematics in a British university would find any difficulty in following the arguments, since the first three chapters, one third of the book, are a lucid development of the essential hydrostatics and hydrodynamics from elementary considerations, and much of the fourth chapter is an introduction to heat-transfer theory. The fifth chapter is concerned with the role of solar and atmospheric radiation in micrometeorology, and in the three subsequent chapters the temperature field, wind structure, and diffusion (including natural evaporation) are discussed. The treatment is theoretical, but Professor Sutton eschews the display of mathematics for its own sake.

To quote the final phrase of the book, micrometeorology is a ' subject in which much remains obscure.' It is also a subject in which it is imperative that we should be able to make predictions, for in widely different technical fields, in peace as in war, processes in the lower atmosphere are becoming quite literally matters of life and death. The merit of the methods described in this book is that they allow predictions which are often astonishingly accurate, and which can as yet be made in no other way, but it cannot be denied that more often than not they offer empiricism and artifice rather than true understanding, a technique rather than a science. The book sets out authoritatively the present position of the subject, and does not seek too obviously to undermine that position: according to temperament and training the reader may applaud or deplore.

The book is well produced in the manner expected of its publishers, but the text is not free from minor blemishes. ' Her Britannic Majesty's Stationary Office ' may be a misprint or a heartcry; a slip on p. 207 in a discussion of temperature profiles might be more misleading.

G.D.R.

The theory of homogeneous turbulence. By G. K. Batchelor. Cambridge Monographs on Mechanics and Applied Mathematics. Cambridge University Press, 1953. Pp. xi, 197; 28 Figs. 25s.

According to a statement on the wrapper, this book is the first of a new series edited by G. K. Batchelor and H. Bondi. Other volumes planned, or in preparation, will deal with turbulent shear flow, hydrodynamic stability, wave mechanics and sound.

Dr. Batchelor's expositions of recent aspects of turbulence theory are well known and in the pages of our *Journal* are to be found some interesting applications of these ideas to the difficult problem of atmospheric turbulence. Although the present book has no direct application to the problems of the atmosphere, it represents a contribution to knowledge which cannot be ignored by the meteorologist. The great complexity of the atmospheric problem has made the semi-empirical approach unavoidable in most instances, but if progress is to continue, a more exact analysis is essential. Such rational treatment is possible at present only if severe restrictions are laid down at the start, and this book is an account of investigations in a very limited field.

Homogeneous turbulence is a mathematical abstraction, an idealization of the type of fluid motion with which we are familiar in the atmosphere or in the laboratory. Dr. Batchelor defines homogeneous turbulence as ' a random motion whose average properties are independent of position in the fluid.' No such motion is known in nature or can be produced in a wind-tunnel, but the state can be approached in experimental arrangements, a fact which allows the consequences of the mathematical study to be examined by physicists. It is generally accepted that all types of fluid motion are subject to the Navier-Stokes equations of motion and the equation of continuity so that, in theory, velocity and pressure in an incompressible fluid can be determined at all points and for all time in any system for which the requisite initial and boundary conditions are known. It is equally well known that exact solutions can be found in a few, relatively simple, cases only, mainly because of the non-linearity of the equations of motion.

There have been many attempts to construct a rational theory of turbulence by the study of perturbations of one or other of the simple soluble motions, but the present book approaches the matter differently. In brief, it is supposed that the initial turbulent state is known statistically, that is, at some given instant the velocity of the infinite fluid, although ' random,' is described by probability laws which are independent of position. The problem then is to determine the probability laws which govern the motion at any later time. In this respect, as the author points out, the subject is very like statistical mechanics, the main difference being that statistical mechanics deals with swarms of particles subject to collision laws and homogeneous turbulence with a continuum subject to certain differential equations, that is, to a complicated balance in which the tendency of inertia (mass) to enhance non-uniformity is to some extent opposed by the smoothing action of viscosity.

Stated in this way, homogeneous turbulence is almost a branch of ' pure ' mathematics, but one of the most attractive features of the book is the skilful way in which Dr. Batchelor has blended the analytical and experimental approaches. The opening chapters contain a difficult but clear and critical discussion

of the mathematical representation of the turbulent field and the general kinematics of homogeneous turbulence. Probably, not everyone will agree with the view expressed, but the author makes his own position clear and states his points unequivocally. The remaining chapters deal with more specialized topics, chiefly, the problem of decay of turbulence in a homogeneous field. Altogether, the book gives a readable and well-balanced account of the work of the Cambridge school, founded and inspired by Sir Geoffrey Taylor, and the impression left on the reader's mind is that of a particularly happy integration of physical insight, experimental skill and mathematical craftsmanship.

The problems of main interest to the meteorologist, sheared motion near a rigid boundary and the mixing effect of the fluctuations are, by hypothesis, excluded. It is possible that something like homogeneous turbulence is to be found in the high atmosphere, but at present we know so little about the details of motion at such heights that few meteorologists would care to put their conclusions in writing. Interest in the study of homogeneous turbulence, however, does not depend on such resemblances, but on the fact that here is a problem which, when defined and analysed by rigorous mathematics, leads to greater insight into the mechanism of turbulence. Dr. Batchelor is to be congratulated on this lucid and timely work.

The layout, press work and reproduction of the diagrams are in accordance with the high standards of the Cambridge University Press, and no further comment is needed.

O.G.S.

Thermal diffusion in gases. By K. E. Grew and T. L. Ibbs. Cambridge University Press, 1952. Pp. xi, 143; 40 Figs. 22s. 6d.

This book deals with that part of the subject of ' molecular ' diffusion that is concerned with gases in which there are temperature gradients. The turbulence of the atmosphere, which extends throughout the troposphere and stratosphere, and perhaps well into the overlying mesosphere, renders molecular diffusion of little importance in general meteorology; hence Dalton's law, according to which, in a mixed gas at rest, the various constituents should decrease upwards in density at different rates depending on their molecular weights, does not have free play, and the permanent constituents of the air are uniformly mixed up to heights of at least 60 km.

However, the rate of molecular diffusion varies inversely as the density, and above some high level – whose position, in the absence of quantitative knowledge as to the degree of turbulence as a function of height, we cannot determine – molecular diffusion may overcome eddy diffusion. If, as is not unlikely, there is a temperature gradient in that region, this circumstance will affect the state of diffusive equilibrium.

From the admirable monograph here reviewed, the meteorologist can learn how ' thermal diffusion,' due to a temperature gradient in a mixed gas, depends on the numerous factors by which this complex phenomenon is influenced. Drs. Grew and Ibbs, who are among the leading experimental workers on the subject, give a clear, readable and concise account of the history, development and present state of this branch of the physics of gases and the kinetic theory. Whatever its bearing on meteorology, readers of this Journal may find much to interest them in the story of the discovery, nature and modern practical applications of this type of diffusion.

S.C.

A century of London weather. By W. A. L. Marshall. London (H.M.S.O.), 1952. Pp. 103; 7 Figs., 29 plates. 15s.

The declared aim of this novel and useful compendium is to provide prompt and accurate answers to inquiries of the type which newspaper reporters and other persons interested in London weather records, present or past, are apt to put to the Meteorological Office. How long is it since we had a March as dry as this year's ? When was the hottest July that anyone now living can remember ? Which is ordinarily the sunniest month ? What is the normal night minimum temperature at the end of August ? Is January ever wholly free from frost ? Does a cold autumn tend to be followed by a hard winter ? On all these and a thousand similar points curiosity can be satisfied by reference to Mr. Marshall's well-indexed tables, figures, diagrams and descriptive text. The information given does not cover the years subsequent to 1949, but possessors of the little volume may bring it up to date without much trouble if they have access to files of the *Daily Weather Report* and *Monthly Weather Report* from January 1950 onwards. There are a few blank pages which could be used for this purpose.

The book is divided into six parts, plus the index. After a brief introduction there is a 37-page section dealing comprehensively with the yearly, monthly and seasonal characteristics of London's climate as exemplified by averages and extremes of air temperature and rainfall over the 109 years 1841-1949 and by sunshine data for the last seven decades of that period. Of special value are analyses giving the frequencies with which seasons broadly classified into various categories of warmth and wetness have been preceded and followed by adjoining seasons of like or opposite type. We find, for example, that after a cool summer there exists a marked tendency towards a cold autumn; that no really cold autumn has heralded a mild winter, and no wet winter a very dry spring.

Next come 12 pages of text, tables and diagrams concerned with visibility and the prevalence of fog in various parts of the metropolitan area. Recent observations from the roof of the Air Ministry building in Kingsway are compared with a series made from the summits of St. Paul's Cathedral and Westminster Tower

through the winter of 1901-2; the conclusion is drawn that while the general obscurity of the atmosphere in central London at that season has decreased decidedly during the last half-century, plenty of room for improvement remains. Visibility, Mr. Marshall tells us, still does not exceed 1·25 mile for 80 per cent of the time from November to January.

Section 4 looks into the question of annually recurring warm and cold spells. Here the author uses five-day means of maximum temperature to test the ' Buchan periods.' He reaches a verdict against their reality so far as London is concerned. This discussion, like most of its kind, loses force through ignoring two highly important provisos laid down by Buchan in his original papers of 1867: (a) that ' the incidence of each of the periods is subject to variation, and in the case of the December period to great variation, from year to year; ' (b) that for the detection of these seasonal anomalies ' five-day means and such mean temperatures as have been " corrected " or manipulated in any way are unsuitable.' How much argument and misapprehension would have been saved had Buchan not been so injudicious as to assign definite dates to ' periods ' which on his own showing were inconstant.

The 39 pages of Section 5 present summaries of temperature and rainfall for each month and year from 1841 to 1949. These refer to Greenwich Observatory until 1870 and to Kew Observatory thereafter, sunshine data being added from 1880 onwards. A prefatory paragraph and table point out the inhomogeneity introduced into the temperature series by the different modes of thermometric exposure – the Airy stand at Greenwich and the north-wall screen at Kew. As compared with the standard Stevenson screen these types of shelter have the effect of expanding and contracting the recorded thermal range respectively. Mr. Marshall shows that over the period 1906-1935 the average difference between the maximum and minimum temperatures was 2·4°F greater at Greenwich than at Kew over the whole year and about 4°F greater from May to August. But these figures are far from evidencing the full extent of the inhomogeneity. A comparison made by the reviewer indicates that from 1871 to 1937 the observed extreme maximum temperature was higher at Greenwich than at Kew by 6°F in 31 months, by 7°F in 19 months, and by 8°F in five months, while the observed extreme minimum was lower at Greenwich than at Kew by 4°F in 31 months, by 5°F in 10 months, and by 6°F in two months. And this despite the fact that Greenwich Observatory is (or was) on a hill-top, whereas Kew Observatory lies near the bottom of a river valley. The apex disparities of 8°F in the maximum readings and 6°F in the minimum readings at these two London stations, a dozen miles apart, represent the normal differences between Penzance and Lerwick and between Torquay and Dundee respectively. Finally, after the index, we have 29 coloured plates setting forth as annual, seasonal and monthly diagrams summaries of the temperature, rainfall and sunshine values detailed in Section 5, plus the day-to-day records of the three elements in individual months that were outstandingly warm and cold, wet and dry, bright and dull. These will have particular appeal to the student of short-term climatic changes.

It could hardly be expected that a work containing such a wealth of statistical material as this would make its first appearance free from factual errors. Perhaps the wonder is that there are not more of them to be pointed out for adjustment in the second edition. The dates 19 October and 16 May given on pp. 27 and 17 for the earliest and latest recorded London snowfalls within the period considered are both incorrect: in 1887 even Kensington had a snow-cover on the morning of 12 October (see *British Rainfall* for that year), and in 1867 the southern suburbs did not escape the heavy snow-squall in which Hermit won the Derby on 22 May. Also inaccurate is a statement on p. 20 to the effect that no ' official ' ground frosts have come to London in July: Greenwich Observatory had grass minimum temperatures of 28°F on 19 July 1863 and 29°F on 16 July 1919. In the table on p. 91 the coldest May night of 1931 is credited with a screen minimum of 48°F at Kew. This, though copied from the *Monthly Weather Report*, is obviously wrong, since the mean minimum for the month was 46·9°F; according to the *Daily Weather Report* 48°F should read 40°F. On p. 77, 29 April with 13·2 hr is entered as the sunniest day of 1889; 1 June with 13·8 hr should be substituted. On p. 71, 12 October ought seemingly to be allowed to share with 13 July the distinction of being the wettest day in 1873, and at the bottom of p. 81 there is an erroneous entry for the maximum daily rainfall of 1903. The snowfall of 27 April 1919 is said on p. 15 to have lain 4 in. deep for a time; actually the depth that evening was 8 in. to 10 in. over most of northern and north-western London and reached 12 in. on Hampstead Heath. The table of ' absolute droughts ' on p. 6 covers only the years 1929-49: reference to p. 16 of *British Rainfall* 1887 and to subsequent volumes of that publication would have enabled the list for Camden Square to be taken back to 1858 with little trouble.

Mr. Marshall adopts a homelier style of writing than is ordinarily found in literature published under the government imprint. Loose syntax abounds. A typical specimen runs (p. 14), ' In 1949 the warmest (April) day at Wealdstone was 84°F on the 16th and the coolest night 28°F on the 10th.' It would be to the advantage of the second edition of the book if all such sentences were amended to read ' The warmest day . . . was the *n*th with *x* °F . . .,' and so on.

E.L.H.

Archiv für Meteorologie, Geophysik und Bioklimatologie, Serie B, Bd III. Vienna (Springer), 1951. Pp. 323; Figs., tables. DM.46.

Volume III, Series B, of the well known ' Archiv ' is dedicated to the General Institute for Meteorology and Geodynamics of Vienna on the occasion of the celebrations in 1951 of the Institute's centenary. With the Institute in the dedication are linked the names of Prof. Dr. V. Conrad and Prof. Dr. H. Ficker, two of the outstanding meteorologists associated with it in the past 40 years, and who reached their 75th and 70th birthdays respectively in the same year.

The dedication points out that the receipt of contributions from all over the world for this special number indicates the respect for the scientific work of the Institute and of Profs. Conrad and Ficker.

The volume includes 23 papers from authors in several European countries, the U.S.A. and India. The subjects dealt with include radiation, surface and upper-air climatology, bioclimatology, meteorological effects in disease, evaporation, climatic change, and glaciology.

Only a few papers can be mentioned individually. Albrecht and Goldschmidt each describe observations of total radiation from a cloudless sky and discuss formulae for the relation between the radiation and the altitude of the sun and with cloudiness. Flöhn discusses the mechanism of the occurrence of westerly winds near the Equator and Reinhard the upper wind observations made by radar on the continental coast of the English channel during the war. Ramdas gives a valuable survey of the investigations at Poona into the meteorology of the lowest few feet of the atmosphere; this paper contains much information on temperature, humidity and wind gradients, on the heat loss from the surface by convection and on the temperature and humidity in various kinds of crop. Ekhart provides a comprehensive climatological study of 50 years' wind observations at Vienna.

Thornthwaite describes a method of measuring evapotranspiration and some calculations of the potential evapotranspiration for different climates, and Lauscher the worldwide distribution of Hill's cooling power. In one of the papers on meteorological effects in disease Berg produces statistics to show that occurrences of lung embolism are not associated with passage of fronts. Prohaska produces evidence that the Antarctic has not experienced any increase of mean temperature such as has occurred over much of the Arctic since the early years of the present century and Winter examines the secular variations of the summer temperature of Vienna with 25-year overlapping means compiled from the 175-year-long series of observations; the values, dated from the last year of the period, were high in the first thirty and last thirty years of the 19th century, fell after 1900 and are now rising again.

G.A.B.

Archiv für Meteorologie, Geophysik und Bioklimatologie, Serie A, Bd. IV. Vienna (Springer), 1951. Pp. 448; Figs., tables. DM.65.

Volume IV, Series A, which is also a special number, contains 31 papers, all of them substantial ones, in its 448 pages. Their subjects include tropical cyclones, theory of water-spouts and tornadoes, gravity waves in the atmosphere, lag in radiosondes, solar effects on the frequency of formation of depressions, dynamics of jet streams, formation of ice crystals in the atmosphere, electric conductivity of the air near the ground, electric currents in the ionosphere, changes in ozone amount associated with sudden ionospheric disturbances, methods of statistical and harmonic analysis in geophysics, atmospheric thermodynamics, turbulence, and seismology.

Again only a few papers can be mentioned individually. H. Koschmieder reviews theories of formation of dust devils and tornadoes and has a detailed analysis with good photographs of a dust devil in process of formation; he regards dust devils as whirls set off as air flows past the edges of obstacles and so accounts for the high proportion of anticyclonically rotating ones and calculates some pressure falls for tornadoes on the basis that the pressure in the centre is at most the same as at the cloud base. W. Bleeker and A. Delver give a new theory of the formation of tornadoes on the basis of the temperature differences between the areas with heavy rain and those without. C.-G. Rossby, in a mathematical paper, explains the formation of jet streams on the basis that stratified air currents assume that distribution of wind velocity with height which corresponds to a minimum value of rate of momentum transfer in the vertical. D. P. McIntyre outlines the main principles of the Chicago school of meteorology and of its leading spirit C.-G. Rossby as devotion to study of the hydrodynamics of the field of flow rather than of pressure. There are two British contributors: Prof. S. Chapman and Dr. R. S. Scorer. Prof. Chapman introduces a new term into ionospheric physics with his equatorial ' electro-jet ' which is an intense electric current flowing eastwards over the sunlit equator invoked to explain the large diurnal range in the horizontal magnetic force at Huancayo and other equatorial observatories. Dr. Scorer outlines his mathematical theory of gravity-waves in the atmosphere and discusses in detail the circumstances in which the waves can be formed.

May we add our congratulations to the Austrian Central Institute and our best wishes for its second century ?

G.A.B.

Berichte des Deutschen Wetterdienstes in der U.S. Zone, No. 42, 1952. Pp. 463; Figs., tables, charts. DM.50.

This number of the series of *Berichte* of the meteorological service of the U.S. Zone of Germany is dedicated to the well-known climatologist Prof. Dr. K. Knoch on his 70th birthday. The volume contains 107 papers, many very short, in its 463 pages.

The subject range is wide; a rough classification list is climatology, bioclimatology, micrometeorology, phenology, agricultural meteorology, radiation, instruments, meteorological factors in health and disease, upper air climatology, meteorological organization.

Taking a few papers almost at random: Dr. K. Keil gives an interesting article on the development of the Library of the U.S. Zone service from its first foundation in 1886 as the Library of the Prussian State

Meteorological service to its recovery after the late war, M. Manig has an article with good charts on the temperature extremes of western Germany, W. Caspar gives the degree-day sums of temperature in Germany for heating purposes and dates of first and last ' heating ' days with good charts, and K. H. Richter discusses the diurnal variation of upper winds over Langenhagen on the basis of a year's radar wind observations.

G.A.B.

Météorologie générale, Tome I. Structure verticale de l'atmosphère. L'atmosphère et les phénomènes de rayonnement. By J. Roulleau and R. Trochon. Paris (Gauthier-Villars), 1952. Pp. 149, 46 Figs. N.p.

This book is without preface, introduction, or apologia, a fact which will no doubt be welcomed by the ordinary reader but which is calculated to make the reviewer work. From the title page it appears to be the first volume of a series of monographs to be published under the authority of the Director of the French Meteorological Service and its style suggests that its main purpose is the instruction of meteorologists within that service.

The first three chapters (80 pages) are concerned with temperature, pressure, and composition of the atmosphere, and in particular with their variation in time at all heights. The treatment is mainly descriptive, and undue emphasis is not placed on surface observations. The last two chapters (50 pages) deal with solar, terrestrial, and atmospheric radiation and associated temperature changes. They are sound but rather uncritical; a knowledge of physics up to about ordinary degree standard is assumed.

The book is hardly likely to displace any in common use in this country, but it will be of interest to anyone concerned in the teaching of meteorology in view of the original and notably concise presentation of the facts in the earlier chapters.

G.D.R.

Soil physical conditions and plant growth. Edited by Byron T. Shaw. New York (Academic Press), 1952. Pp. xv, 491; 97 Figs., 65 tables. 70s. 9d.

This monograph, prepared under the auspices of the American Society of Agronomy, has been written by nine authors in five main sections: soil as a physical system; mechanical impedance and plant growth; soil water and plant growth; soil aeration and plant growth; soil temperature and plant growth. The book will serve its intended readers admirably, but its meteorological content is so slight as to be negligible. The excellent sections on soil water and temperature will, however, provide useful background information for those meteorologists who are interested in the effect of weather on plants.

H.L.P.

Altimeter errors near mountains. By R. S. Scorer. Unpublished MS. Pp. 32; Figs., tables.

Our knowledge about airflow over mountains is very limited and before much progress can be made in this subject a good deal of practical data in the field must be obtained. Observations have been made by Förchtgott in Czechoslovakia and also by the Sierra Wave Project in California but, so far, little has been done in this country. This paper contains an account of a series of experiments made in North Wales during Sept. 1951 by members of Imperial College and various Gliding Clubs to measure the pressure field upwind of mountains. The results were inconclusive, mainly owing to instrumental failings, but no doubt the experience gained will prove invaluable to Dr. Scorer and his colleagues in their efforts to obtain further data in subsequent work.

The gliders were equipped with sensitive altimeters to measure pressure and their true heights were determined simultaneously from double-theodolite observations from the ground. In addition, the pilots measured temperatures at various levels throughout the flights. The difference between the reading of static pressure and the hydrostatic pressure, calculated upwards from the surface using the temperature and height readings, is the ' altimeter error.' In this work it was only about 1 mb but, with stronger winds, up to 10 mb would be anticipated from theoretical considerations. If the distribution of this altimeter error upwind of the mountain could have been measured, a useful check would also have been obtained on Dr. Scorer's application of perturbation theory to this problem and the validity of the assumptions it contains. Unfortunately it was not known by the experimenters, before the measurements were made, that the standard sensitive altimeter possesses a large lag and, as the sinking rates of the gliders were not measured, the resultant uncertainty of the pressure readings invalidated the measurements. One wonders whether a slow-flying aircraft keeping constant indicated height and being observed by kinetheodolites would not be a better experimental tool in the future.

Apart from a review of the theory of airflow over hills and a discussion of the possibility of model experiments in wind tunnels this paper is therefore only of limited interest. Indeed from its layout it would appear that it is intended primarily for interested members of Imperial College and the Gliding Clubs. The conclusions in particular might be better arranged.

R.J.M.

Obituary Notices

The Reverend H. NORMAN EDGE

The Reverend Henry Norman Edge, who died at the age of 61 on 10 March 1953, had been an enthusiastic student of meteorology and the allied sciences since his boyhood. Ordained after educational courses at the Liverpool Institute and the University of Birmingham, he held his first curacy at New Cross, London. Successive rectorships later took him to Fifehead Neville, Dorset, to Kettlestone, Fakenham, Norfolk, and finally to Castle Carrock in the Carlisle diocese, from which living he was compelled by ill health to retire in 1952. Mr. Edge could claim some professional experience as a meteorologist. He was at one time an assistant at the headquarters of the British Rainfall Organization under Dr. H. R. Mill, and during the 1914-18 war he joined the meteorological section of the Royal Naval Air Service. At another period he was appointed lecturer in meteorology at the Lancashire and National (Navy League) Sea Training Homes. He played a prominent part in the establishment of the first meteorological station at Wallasey and did much work at the Bidston Observatory, Liverpool. He had also been secretary to the Liverpool branch of the British Astronomical Association. His gift of clear and simple exposition conjoined with a friendly manner and a lively sense of humour brought his services as a lecturer on meteorology, astronomy and volcanology into considerable popular demand. Mr. Edge was a Life Fellow of this Society, elected on 21 June 1916.

E.L.H.

The Reverend C. DECKER

The Reverend Charles Decker, who had held Fellowship in the Society since 18 December 1946, died suddenly at the age of 81 on 22 February 1952. A native of Liverpool, he embarked on a business career there after receiving his education at Sedbergh. A few years later he emigrated to the United States, where he was occupied successively in journalism and banking. When about 60 he went to Rome to be trained for Roman Catholic priesthood at the Beda College. On ordination he was appointed to the Leyfield Orphanage, West Derby, Liverpool. Here he passed the remainder of his active life, retiring towards the close of his eighth decade. Father Decker was an enthusiast in observational meteorology, and had kept regular records of temperature and rainfall at his Lancashire home over a number of years.

E.L.H.

Corrigenda to April 1953 issue

Gordon Manley, p. 198, Table 2—

Column D, 4th line, *for* 54-19 *read* 51-19.

Gordon Manley, p. 253, Table 2—

Adopted mean for January 1730, *for* 38·5 *read* 39·3.

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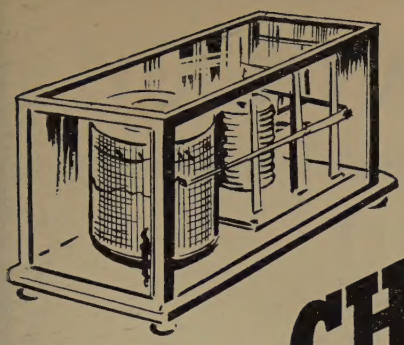

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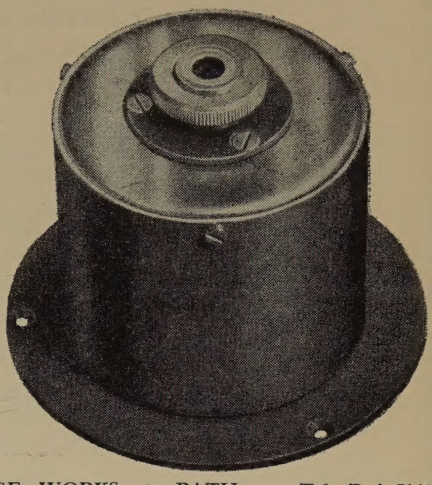

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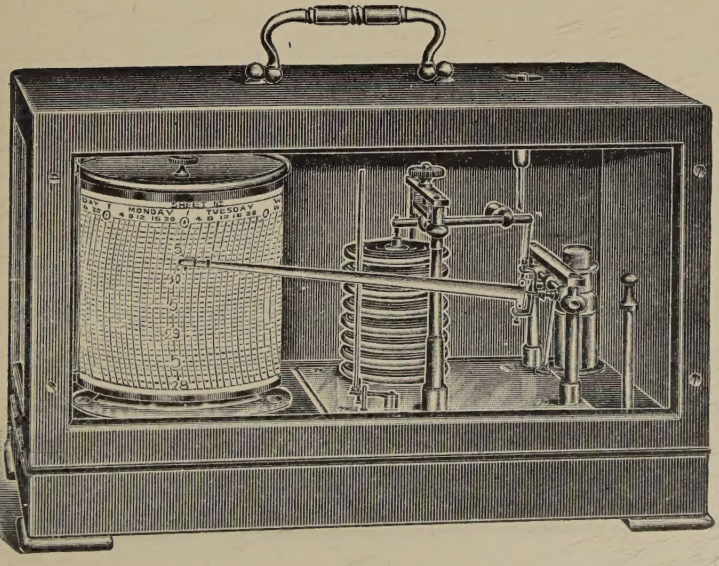

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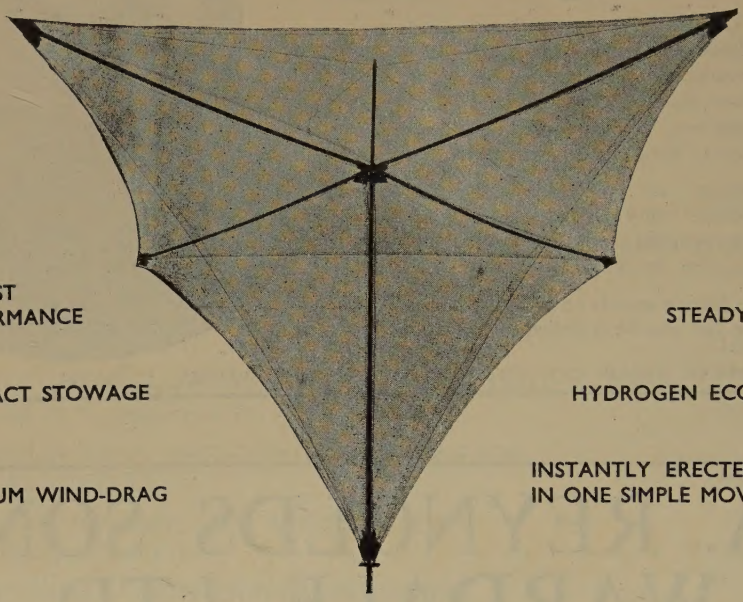

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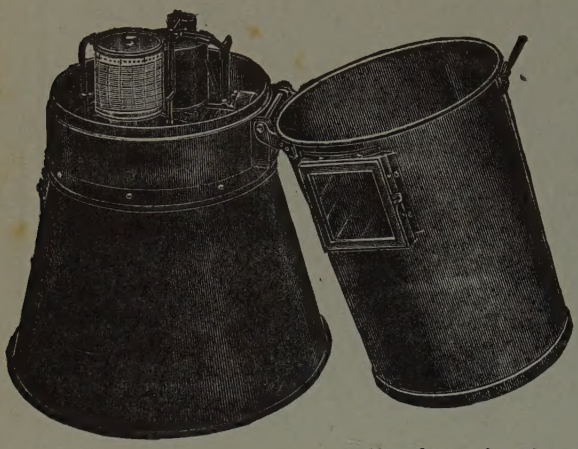

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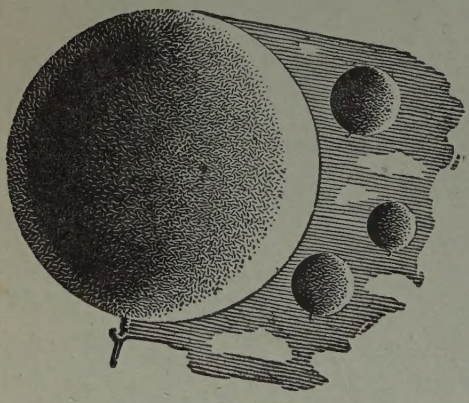

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